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ON ERRORS OF OBSERVATION.[1]

BY PROFESSOR JAMES MCKEEN CATTELL,

Columbia College.

Currents of thought often arise at different sources, and flow on for a long way before they mingle. This has been the case with the investigation of errors of observation in physics and in psychology. On the one hand methods for securing the nearest approximation to the true value from discordant observations have been studied by many of the most eminent mathematicians and physicists since the revival of learning. On the other hand the accuracy with which the external world is perceived has always been a central subject in psychology, and in the development of experimental psychology no portion has received more attention than the perception and comparison of differences in intensity. It has, however, to a considerable extent been overlooked that physics and psychology are concerned with the same phenomena. This is not surprising, as the points of view of the two sciences are different. Physics seeks to eliminate errors of observation; psychology seeks to study their nature. But the time has now come when each science should profit from the progress of the other. Physical science can better eliminate errors of observation by learning what is known of their cause and nature. Psychology will gain greatly in clearness and accuracy by using the methods of physics and mathematics.

The errors of observation with which physics and mathe-

[1] Read at the meeting of the American Psychological Association, Philadelphia, 1892.

matics have dealt are variable errors, such errors as would occur were each error composed of a very large number of comparatively small and independent errors, equally likely to be positive or negative. In this case the average of the observations is the most likely value, and its approximation to the true value is measured by the dispersion of the errors, and increases as the square root of the number of observations. In two important respects the mathematical theory needs to be supplemented by psychological experiment. In the first place, constant errors are entirely beyond the range of the method of least squares, and yet these are evidently more dangerous in physical observations than variable errors. Thus, for example, in the case of the personal equation of the astronomers, the variable error of an observer can be reduced to any desired extent by increasing the number of observations. But it was found on comparing the observations of different observers that they had constant errors far more serious than their variable errors. It was (and apparently is still) thought that the constant error of an observer becomes a variable error when the observations of several observers are combined. It is very unlikely that this is the case. The uniformity of the processes of perception and movement is greater than their variability. We may feel confident that the combined personal equations of all the astronomers would be subject to a constant error which cannot be eliminated by physical or mathematical science. But such constant errors depend on fixed psycho-physical conditions, and can be measured by the psychologist.

In the second place it may be urged that the theory of probability can only give a rough and ready account of the distribution even of variable errors. In measuring an inch an error of a mile will not occur, and a negative error of a mile is inconceivable. The probability assigned to such errors by theory is, indeed, extremely small, but the same probability is assigned to positive and negative errors, and they are not equally likely. It would seem that as a rule positive errors are more likely than negative errors. In measuring an actual inch, a positive error of two inches might occur, a negative error of the same size cannot occur. In ordinary errors of observation a corresponding preponderance of positive errors may be expected, and a correction for such excess must be empirically determined. The same holds for the averages which are so widely used in statistics. Thus, if the average weight of men be 150 pounds, men weighing 300 pounds occur, men weighing 0 do not occur. The average is not identical with the median, as required by the theory of probability. The assumption made by the mathematicians,

that an error is composed of a very large number of comparatively small and independent errors, cannot be admitted by the psychologist. If the fiction of indefinitely small errors be accepted at all, the elemental errors cannot be regarded as independent, but are interdependent and occur in groups. The distribution of errors will not follow simple and universal formulæ, but the greater our knowledge the more complicated will the formulæ become, and they will be as numerous as there are observers and observations. The deductions of Laplace and Gauss are of the greatest importance, but it should not be forgotten that the laws of nature cannot be invented, they must be discovered. It is within the province of psychology to supply physics with the formulæ it requires for eliminating errors of observation in special cases.

Turning now to what psychology can learn from physics, we find that the variable error of the method of average error and the probable error (or h as used in Germany) of the method of right and wrong cases are the error of observation of physical science. We may ask, why should there be an error of observation? Why should not the same stimulus be accompanied by the same sensation? The natural answer is that the conditions do not remain the same. In the first place the stimulus itself cannot be kept exactly constant. Lights are always variable, and sounds and touches cannot be exactly reproduced. Temperatures and smells are especially inconstant. Weights may remain nearly the same, but the manner of lifting them is always different. We have, therefore, a variable stimulus which in part accounts for the variation in sensation. In the second place the nervous mechanism is constantly changing. The sense organ is rhythmically exhausted and restored, and is subject to various irregular alterations. The nerves and paths of conduction in the brain would transmit more or less of the energy of the stimulus according to their ever changing condition. Lastly, the brain centres immediately concerned with perception alter greatly in metabolism. These latter changes are best known to us on the side of consciousness; there is a more or less regular rhythm in attention, and very numerous irregularities due to fatigue, interest, inhibition, etc. These sources of variation will sufficiently account for the fact that the same sensation does not recur. They are, indeed, so numerous and to a certain extent so independent, that they justify roughly the assumption of the mathematician, and the results of experiments show that the errors are in a general way distributed as required by the theory of probability.

In psycho-physical experiment two magnitudes are perceived and compared. The combined error of perception

would be larger than a single error of perception, being the square root of the sum of the squares of the separate errors, or nearly the error in a single case multiplied by the square root of two. We have further the errors of memory and comparison. The analysis of these factors at the present time would be very difficult, but I believe they would simply increase the variable error of observation, and introduce additional constant errors. This is not the view taken by Fechner, Müller, Wundt and others, to whom we chiefly owe the development of psycho-physical research and theory. They maintain that there is a threshold of difference, and when sensations differ by less than this amount there is no difference in consciousness, Fechner does not question the application of the probability integral to the comparison of magnitudes,[1] on the contrary it was he who first applied it to the method of right and wrong cases. He argues that a difference in the stimuli smaller than the threshold might be made apparent in consciousness by the error of observation, and would give the preponderance of right cases required by theory. But in about one-seventh of his trials he was doubtful as to which of the weights used by him was the heavier, and holds that in these cases the difference in the weights and the error of observation combined fell within the threshold, and that there was no difference in consciousness.

Prof. Fullerton and the writer[2] made experiments with lifted weights similar to Fechner's. In one series of 3000 experiments in which the probable error was much the same as Fechner's, the observers were doubtful 23% of the time, but on guessing which of the weights was the heavier they were right 62½% of the time. This is the percentage of right cases required by the theory of probability, on the supposition that the differences in consciousness follow Gauss' formula, and we may conclude that the difference in consciousness always exists and affects the course of mental life, even when it is so small that it cannot be detected.

Another case in which German psychologists have run counter to the theory of probability is in the assumption of a just noticeable difference. According to the theory of probability the apparent difference in sensation and the probability of correct judgment tend to increase continuously as the difference between the stimuli is made greater, but it is

[1] As implied by Peirce and Jastrow in their important paper (*On Small Differences in Sensation*; National Academy of Sciences, III. [1884]), which for the first time denied the supposed fact of the threshold.

[2] *On the Perception of Small Differences;* Univ. of Penn. Press, 1892. The present paper is largely based on this monograph.

entirely arbitrary to choose one difference and call it just noticeable. A difference in the stimuli can be found which will be obscured by the error of observation 1 time in 10, or 1 time in 1000, but no difference can be called just noticeable, meaning that it and larger differences will be correctly distinguished, while smaller differences will be indistinguishable. In actual experiments Prof. Fullerton and the writer found that the difference fixed on by the same observer under changed conditions as just noticeable was not at all proportional to the error of observation, and with different observers the difference which they considered just noticeable in no way measured their accuracy of discrimination. In the many researches in which the method of just noticeable difference has been used, the just noticeable difference fixed on by the observer has probably been determined partly by his general knowledge of his error of observation (the difference he would seldom mistake) and partly by association, he choosing an apparently equal difference.

The last application of the theory of probability which I wish to make concerns the relation of the error of observation to the magnitude of the stimulus. The algebraic sum of a number of variable errors tends to increase as the square root of the number. In measuring the base line of a survey the variable error of observation increases as the square root of the length of the line. It seems to me the same relation might be expected to hold in a general way when the length of a line is estimated by the eye or compared with another line. Or to take another example, if we estimate one second of time and repeat the trial four times, the algebraic sum of the four variable errors, or the combined error in estimating the four seconds, will tend to be twice as great as the error in estimating a single second. If we estimate or compare the four seconds continuously, the same elements would to a considerable extent be present, and we might expect an error twice as great as in estimating a single second—not four times as great as required by Weber's law.[1] The error in estimating each of the several seconds might and doubtless would be different, and in the case of intensive magnitudes equal objective increments would seldom or never be accompanied by equal changes in consciousness, nor be subject to equal and independent errors. The theory of probability only considers the simplest and most general case. We must use all the knowledge we have

[1] Constant errors increase in direct ratio to the magnitude, and would tend to follow Weber's law. But, curiously enough, constant errors have not been supposed by the psychologists to follow Weber's law. As a matter of fact "constant errors" are very inconstant and difficult to investigate.

as well as our theory, and the general formula must be adjusted to each special case.

In attempting to pull a dynamometer twice with the same force we do not compare the movements as we proceed, but the final result, and if the force were near the limit of our strength, the error might be less than for a smaller magnitude. We should expect a post-office clerk to judge very light weights better than a blacksmith, a blacksmith to judge heavy weights the better. We should expect to discriminate lights best within the range of ordinary daylight, and sounds best within the range of the human voice. Such results would be contrary to Weber's law, but are simply factors additional to the summation of errors required by the theory of probability. The relation between the error of observation and the magnitude of the stimulus will differ for each stimulus and for each observer, and will not remain constant even for the same stimulus and the same observer. But the usual increase of the error of observation with the magnitude of the stimulus is accounted for in a satisfactory manner by the summation of errors, and I should substitute for Weber's law the following: *The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case.*[1]

It may be asked if this view be correct, why do the results of researches confirm Weber's law? As a matter of fact Weber's law has not yet been confirmed exactly by any careful research, the error of observation usually becoming larger as the magnitude of the stimulus is taken larger, but almost always more slowly than in direct proportion to the magnitude. The attempt has been made by Fechner, Wundt, Helmholz and others to explain away the variations by additional hypotheses, but it is universally admitted that the validity of a law or hypothesis decreases as the number of subsidiary hypotheses increase.

I venture to think that it is an open question whether in the researches hitherto made the error of observation increases more nearly as the magnitude or as the square root of the magnitude. Researches in which the method of just noticeable difference has been used do not of necessity measure the

[1] Prof. Fullerton pointed out at the meeting of the association that the conditions which made the first fractional or elemental error positive or negative might make the following error tend in the same direction. So far as such a tendency is present the error of observation would increase more rapidly than the square root of the stimulus, and more nearly in direct proportion to it (Weber's law).

error of observation at all. The variation in adjusting the just noticeable difference would roughly measure the error of observation, but this has been neglected. All the researches on lights with which I am acquainted[1] (excepting that by Prof. Fullerton and the writer) used the method of just noticeable (or more than noticeable) difference. Now it is natural enough (considering its elasticity) to make the just noticeable difference within the range of ordinary daylight proportional to the intensity of the light. We see the same objects more or less brightly illuminated, and should tend to regard the differences in shade and color as equal differences, whatever the intensity. It may also be remarked that the mechanism of the eye (accommodation of the pupil and sensitiveness of the retina) tends to obliterate objective differences in brightness. Further in all these researches on lights (excepting Merkel's) the lights were side by side, and the time of exposure was not limited. In such a case the error of observation becomes much obscured, and almost any result can be obtained.

I venture to maintain this conclusion even against the very careful research by König and Brodhun, which supports Weber's law for a considerable range of intensity. It is especially difficult to adjust a just noticeable difference when the areas of light are very small, and for colors not usually seen. König and Brodhun found the just noticeable difference for different colors of apparently the same intensity to be the same (ca. $\frac{1}{75}$ of the light). Previously with much the same methods Lamansky found the just noticeable difference for red $\frac{1}{70}$, for yellow and green $\frac{1}{286}$, for violet $\frac{1}{106}$, whereas Dobrowolsky found for red $\frac{1}{14}$, yellow $\frac{1}{46}$, green $\frac{1}{59}$, violet $\frac{1}{288}$-$\frac{1}{67}$. The three researches were carried out in Helmholz' laboratory, and we may well be at a loss to draw any conclusions from such discordant results.[2] Perhaps the two best researches with lights have been carried out by Aubert and by Müller-Lyer. Both of these writers think their results do not support Weber's law.

It is not necessary in this place further to review and compare results of researches on lights and other stimuli. If it be admitted that the just noticeable difference be not proportional to the error of observation, the amount of work to be

[1] Bouguer, Lambert, Arago, Masson, Fechner, Volkmann, Aubert, Helmholz, Plateau, Delbœuf, Kraepelin, Dobrowolsky, Lamensky, Breton, Ebbinghaus, Merkel, Lehmann, Neiglick, Schirmer, Müller-Lyer, König and Brodhun.

[2] More especially as Helmholz, in the revision of his *Physiologische Optik*, does not even mention Lamensky and Dobrowolsky. Nor does he refer to work not done in Berlin.

considered would be greatly reduced. Further, many researches by the method of average error and right and wrong cases have only a tolerable validity (*e. g.*, Fechner's and Merkel's) because the observer knew the relation of the stimuli before comparing them. In other cases (*e. g.*, with tastes, temperatures, touches and sounds), the stimuli have not been measured in a satisfactory manner. I believe the various researches are so disparate, having been made by so many observers (often young men working for a degree) and by such varying (and in many cases inadequate) methods, that the only general conclusion which can be drawn is that the error of observation tends to increase as the stimulus is made larger and usually more slowly than in direct proportion to the stimulus.

Before concluding I wish to notice the relation between the error of observation and the estimation of mental intensity. It has commonly been assumed that the variable error and the probable error (or h in Germany) are proportional to the just noticeable difference. The just noticeable difference has further been used to measure the intensity of sensation. The just noticeable difference is thus used ambiguously, on the one hand as a difference equally likely to be correctly perceived, on the other hand as a difference accompanied by an apparently equal increment in sensation. I entirely question the application of the error of observation to the measurement of the intensity of sensation. Supposing the intensity of sensation to be measurable, it may increase as the stimulus or (conceivably) as the logarithm of the stimulus, while the error of observation may be any other function of the stimulus[1] When it is evident that the error of observation may be increased or decreased in many ways without greatly altering the apparent intensity of sensation, I cannot understand how it has come to be used as a unit suitable for measuring the intensity of sensation. The error of observation is a physical quantity, a function of the intensity, area, duration, etc., of the stimulus, of the condition of the nervous system, and of the faculties, training, attention, etc., of the observer. That it should increase with the magnitude of the stimulus, and tend to increase as the square root of the magnitude, seems to me a natural consequence of the summation of errors. But I see no necessary connection between the supposed fact that the error of observation increases in direct proportion to the stimulus and the consequence which has been drawn from it that the intensity of sensation increases as the logarithm of the stimulus.

[1] This was noticed by G. E. Müller in 1879 (Zur Grundlegung der Psychophysic, p. 79-80).

The measurement of the intensity of sensation is not out of the question because the error of observation cannot be used as a unit. The attempt is made to accomplish this when for different intensities sensations are adjusted midway between two others, when they are made apparently half or double others, or, lastly, when they are made just greater or less than others in the sense that the difference in sensation is apparently equal. The question here is whether we do in fact judge differences in the intensity of sensations, or whether we merely judge differences in the stimuli determined by association with their known objective relations. I am inclined to think that the latter is the case. I find it comparatively easy to adjust one time, length of line or weight midway between two others, much more difficult to judge when one light or sound is midway between two others, and almost impossible to judge one temperature or pain midway between two others. The difficulty of making a decision increases as the objective relations are less familiar, and I believe that my adjustment is always determined by association with the known quantitive relations of the physical world. With lights and sounds, association might lead us to consider relative differences as equal differences, and the data would be obtained from which the logarithmic relation between stimulus and sensation has been deduced. With the force, extent and time of movement, Prof. Fullerton and the writer have shown that our estimates tend to follow the objective relations. But, in any case, if we merely judge the relations of objective magnitudes by association, we have no basis whatever for determining a relation between physical energy and mental intensity.

I conclude, consequently, that we cannot measure the intensity of sensation and its relation to the energy of the stimulus either by determining the error of observation or by estimating amounts of difference. The most natural assumption would seem to be that the intensity of sensation increases directly as the energy of the brain changes correlated with it. The relation between the energy of the brain changes and the physical stimulus is a physiological question. This conclusion does not mean, however, that psycho-physical research is valueless. On the contrary it is an important contribution to the science of psychology, whence its application will be extended to physical science, to art, to medicine, to pedagogy and in other directions.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

EDMUND C. SANFORD, PH. D., *Director.*

I.

INTRODUCTION.

In beginning the publication of this series of minor laboratory studies, a word of explanation as to their nature seems appropriate. The aim of the laboratory in Clark University is original research, not only for itself, but as a pedagogical means. Many students, however, come to us from institutions in which psychological laboratories have not as yet been founded, and sometimes with no training in experimental science at all, and are therefore more or less unfitted to enter at once upon research work. For this reason the first year in the laboratory is devoted largely to practice experiments, such as those collected in the Laboratory Course already published in part in this *Journal*; but an essential part of each student's work during the year is the execution of some small and definite piece of experimental research, assigned by the director of the laboratory and worked out, after some months of general laboratory practice, under his supervision and generally with his active assistance at the start. The first six of the following papers give the results of such studies. The greater number of papers published under the title of "Minor Studies" will be of this character, but others will also be included (as in this instance that on the Pendulum Chronograph) when their length and subject matter are fitting.

ON THE DISCRIMINATION OF GROUPS OF RAPID CLICKS.

BY THADDEUS L. BOLTON.

With reference to groups of rapid clicks several questions may be asked, among others the following: 1. Is the rate at which the clicks of the group fuse into a continuous tone or noise dependent upon the number of clicks in the group, so

that a small group can be recognized as composed of discrete clicks when a larger group cannot?[1] 2. In the perception of a group that does not fuse, is there a tendency for any of the clicks to drop out, as Hall and Jastrow found that they tended to do when counting was added to simple perception? 3. Can the presence of one more or one less click be recognized when successive groups of different number of clicks are given, and under what conditions as to rate and number? The experiments about to be described have immediate bearing on the last of these, but throw more or less light also on the first and second.

Apparatus and Methods. In such experiments as these, the great difficulty is to get an instrument that will produce a sound of the greatest possible clearness and definition in controllable number and at regular intervals. The required sound was found in the click of the armature of a time-marker of the pattern furnished by the Cambridge Scientific Instrument Co. This sound was produced at the closing of the circuit; the sound at the break was avoided by making the armature react against a piece of soft India rubber. Though the avoidance of this break sound was not absolutely complete, it was not sufficiently loud to interfere with that at the make, and in general was wholly unnoticed. All that was required now to get a regular succession of uniform clicks was to make and break the circuit at regular intervals. This was accomplished by causing a heavy pendulum to draw a projecting platinum point across a series of equal brass plates separated by pieces of hard rubber of uniform thickness. Each plate was connected by a wire with a switch-board, which made it possible to add or subtract one or more of the clicks by throwing one or more of the plates into or out of connection with the time-marker. What was needed for the experiment, however, was not one series of clicks, but two separated by a short interval. To accomplish this, a second platinum point was attached to the pendulum at a suitable distance behind the other. From these platinum points a wire ran up the pendulum bar into a mercury cup opposite its centre of oscillation, thence to the battery (12 Leclanché cells), to the time-marker, and to the switch-board, thus completing the circuit. The pendulum used was like that devised by Drs. Bowditch and Warren, of which a full description may be found in the *Journal of Physiology*.[2] The especial value of such a pendulum for this work lies in the fact that its

[1] Suggested, but not worked out to any extent, by Hall and Jastrow. *Mind*, XI. 1886, 58.

[2] Vol. IX. page 29.

velocity in the different parts of its arc varies less than that of ordinary pendulums. To prevent the friction of the platinum points from affecting the rate of the pendulum, and also to insure a better contact, they were attached to brass wire springs. The pendulum swung each time from a fixed position, where it was held till the required instant by a magnet. To prevent the points making contact at the backward swing of the pendulum, the set of brass plates was attached to a board which was hinged at the bottom and could be withdrawn when the pendulum swung backward. When this board was pressed forward so that the brass plates were in a position to be touched by the platinum points, it broke the circuit through the magnet just mentioned and released the pendulum.

In order to time the apparatus the time-marker was made to write upon a drum along with a Deprez signal driven by a tuning-fork of 100 vibrations per second. At the first setting up of the apparatus ten clicks were used and occupied in all .11 sec. They were consequently separated from one another by intervals of .011 sec. The apparatus was timed very frequently to ensure constant rate. In the later experiments the pendulum was made to swing through a greater arc. This decreased the interval between the clicks to .0065 sec., and in the last experiments the interval was made .0075 sec. The interval between the two groups of clicks was about .25 sec. This interval varied slightly in different portions of the experiments. The amount of variation in the velocity of the pendulum during the time it was passing the plates could not be detected in the tuning-fork record. In all the experiments the time-marker, besides giving the clicks, inscribed its motions on a smoked drum, by means of which an objective record was kept of what had actually happened, and any failure in the instrument was instantly detected.

In making experiments the operator was seated before the pendulum, managed the switch-board with one hand and brought the brass plates into position with the other, while the subject sat near the time-marker and in such a position as not to see the essential manipulations of the operator. The operator told his subject that there would be a certain number of clicks in the first group, *e. g.*, four, and that in the second group there would be either four or five. The subject was required every time to judge which it was; no answers of "doubtful" were permitted. It was not difficult by means of the switch-board to connect or disconnect a wire while the pendulum passed from the position of contact for one point to that for the other, and any failure to do so was shown by the drum record just mentioned.

Results of the Experiments. The experiments were made upon three subjects, and all numbers from four to ten were used as standards and compared with the numbers greater or less by one. About twenty-four observations were made upon each standard, beginning with four and going up to ten. The order was then reversed, beginning with ten as the standard, and about the same number of observations was made upon all numbers down to four. The following table gives the number of observations and percentages of errors for all the subjects and all the standards.

TABLE I.

Showing number of observations and percentage of errors for all the groups of clicks from three to ten. Interval between the clicks, .011 sec.; between the two groups of clicks, .25 sec. The standard always came first and was compared with either the next higher or the next lower number, but in separate sets of experiments; both higher and lower were not used in the same set.

	H.		S.		B.	
Groups to be Compared.	No. of Observations.	Percentage of Errors.	No. of Observations.	Percentage of Errors.	No. of Observations.	Percentage of Errors.
4—3			48	6.	36	5.
4—5			74	13.	72	12.
5—4	68	10.3	79	24.	48	12.5
5—6	50	26.	58	24.	45	11.
6—5	56	16.	62	16.	46	13.
6—7	54	16.6	53	30.	48	27.
7—6	53	15.1	54	29.	44	18.1
7—8	58	19.	54	28.	48	18.7
8—7	49	6.1	55	13.	48	4.1
8—9	67	20.8	62	33.	48	20.
9—8	52	7.6	46	26.	48	16.
9—10	52	13.4	83	32.	72	18.
10—9	46	19.5	46	33.	54	20.3

From this table it will be seen that on most of the standards, excepting the judgments of S., the percentage of right judgments was greater than 75, the proportion at which knowledge may be assumed to be the basis of the judgment. It is possible then to recognize a difference of one in any number of clicks below ten when the clicks are separated by an interval of .011 sec.

The table also shows that the percentages of errors are greater generally for the greater than for the less number of clicks. This will be more apparent by grouping the percentages on several standards and taking the average of each group.

	H.	S.	B.
First group (standards 4 and 5),	17.2	16.7	13.5
Second group (standards 6, 7 and 8),	13.4	23.2	16.2
Third group (standards 9 and 10)	15.3	31.2	18.5

The strain upon attention was very great, and if any disturbance occurred at the moment the pendulum swung, the judgment was impaired. Very frequently the subject was asked what his opinion of the correctness of his judgments was, or whether any disturbance had distracted his attention. The answers were recorded, and a close correspondence was found to exist between these facts and the accuracy of his judgments. Some observations were taken upon two and three as standards, with an occasional error. With these standards the strain upon the attention was even greater than with larger numbers, and any disturbance was more likely to impair the judgment. In general if judgments could be made immediately they were always more satisfactory; delay was fatal.

It was stated above that the experiments were begun with four as the standard and carried through all the numbers up to ten. The reverse order was then followed to compensate for practice. The results of both are united in the table above. In the second half of the observations the percentages of errors were very much decreased by practice. In the first half H. made 69 errors, S. 113, and B. 61; in the latter half H. made 25 errors, S. 87, and B. 36. On several standards in the latter half, H. and B. made no errors at all. The tendency was to judge more different than alike, though the standard was followed by an equal number, i. e., was repeated, as frequently as by a different number. When the standard was followed by an equal number, H. made 67 errors, S. 96, and B. 63; when the standard was followed by a greater or a less number, H. made 27 errors, S. 87, and B. 39.

In all these experiments the standard had been made to precede the number to be compared. To prevent the possibility of there being a difference in the second group which could not be detected by the record on the drum or the measurements with the tuning-fork, and which served as the basis of judgment, observations were taken in which the number of clicks in the second group remained constant and the number in the

first varied. The standard was eight. The results do not differ from those in the table. S. made 17.5% of errors in 128 judgments, and B. 15.6% in 64 judgments.

Some observations were also taken in which the standard came indiscriminately first or last, with the result that in 64 observations there were 7.8% of errors. The interval between the clicks was .011 sec. The standards were 7 and 8. These experiments are not included in Table I.

All the subjects remarked early in the experiment that when the standard was compared with a less number, they experienced less difficulty in detecting the difference than when the standard was compared with a greater number. The actual numbers of errors from Table I., classified with reference to this point, are found in Table II.

TABLE II.

Giving the number of observations upon the standard and lesser group, and upon the standard and greater group. Conditions as in Table I.

SUBJECT H.

STANDARD AND LESSER GROUP.				STANDARD AND GREATER GROUP.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
5—4	68	5	2	5—6	50	8	5
6—5	56	6	3	6—7	54	8	1
7—6	53	5	3	7—8	58	9	2
8—7	49	1	2	8—9	67	11	3
9—8	52	3	1	9—10	52	5	2
10—9	46	6	3				
Totals,	324	26	14		281	41	13
Percent. of Errors,		8%	4.3%			14.5%	4.6%

TABLE II.—*Continued.*

SUBJECT S.

STANDARD AND LESSER GROUP.				STANDARD AND GREATER GROUP.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
4—3	48	2	1	4—5	74	6	4
5—4	79	11	6	5—6	58	8	6
6—5	62	5	5	6—7	53	6	10
7—6	54	9	7	7—8	54	8	7
8—7	56	4	3	8—9	62	9	12
9—8	46	7	5	9—10	83	13	14
10—9	46	8	7				
Totals,	391	46	34		384	50	53
Percent. of Errors,		11.5%	8.6%			13%	13.1%

SUBJECT B.

STANDARD AND LESSER GROUP.				STANDARD AND GREATER GROUP.			
Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Lesser Group.	Groups of Clicks.	No. of Obser.	Errors on Standard.	Errors on Greater Group.
4—3	36	2	0	4—5	72	3	6
5—4	48	5	1	5—6	45	2	3
6—5	46	4	2	6—7	48	6	7
7—6	44	6	2	7—8	48	4	5
8—7	48	2	0	8—9	48	4	6
9—8	48	8	0	9—10	72	7	6
10—9	54	10	1				
Totals,	324	37	6		333	26	33
Per Cent. of Errors,		11.4%	1.8%			7.8%	9.9%
Grand Totals,		109	54=163			117	99=226

This table means, in the case of H., for example, and the group of clicks marked 5—4, that when he was given by the operator 68 tests, half of which consisted of a group of 5 clicks followed by another group of 5, and half of a group of 5 clicks followed by a group of 4, his description of what had been given him was wrong seven times: five times, when he

was given 5—5, he called it 5—4, and two times, when he was given 5—4, he called 5—5. Similarly with the standard and greater group the first line means that when he was given 50 tests, half being 5 clicks followed by 5 and half 5 followed by 6, he eight times mistook 5—5 for 5—6, and five times mistook 5—6 for 5—5.

The total number of errors upon the standard and lesser group are very much less than upon the standard and greater group (163 to 226), this difference being due almost entirely to the very large number of errors (99) upon the greater group. With two subjects the number of errors upon the greater group is greater than upon the standard, and with all three subjects the number of errors upon the lesser group is decidedly less than upon the standard.

This concentration of the errors was quite unexpected, and seemed so remarkable that the experiment was varied for further investigation of the point.[1] The subject was given a standard followed by the same number, or by a less or greater number, thus making three answers possible. This would make a difference between the greater and lesser groups twice that between either group and the standard. On this account, with standards less than ten, the confusion of the greater with the lesser group, and *vice versa*, was very exceptional.

[1] In the discussion that followed a brief report of these experiments given before the American Psychological Association at its Philadelphia meeting, Prof. Jastrow objected to the description of this result as "unexpected," since Dr. Hall and he had noticed the same in the course of their experiments on rhythm (above referred to). That they did notice this peculiar constant error I can well believe, but no mention of it sufficient to cause anyone to expect it here is to be found in the very condensed account of their experiments. E. C. S.

TABLE III.

Giving the number of observations and the percentages and numbers of errors, when the standard was compared with the same number or with a greater or a less number. The interval between the clicks was .011 sec., and between the standard and comparison groups .25 sec. The standards were 7, 8 and 9.

	Lesser Group.			Standard.			Greater Group.		
Subject.	No of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.
H.	36	3	8.3	36	6	16.6	36	8	22.2
S.	21	3	14.2	21	8	38.	20	8	40.0
B.	24	1	4.1	24	6	25.	24	8	33.3
Total.	81	7	8.6	81	20	24.7	80	24	30.0

Total No. of obser., 242; total of errors, 51; percentage of errors, 21.

TABLE IV.

Conditions as in Table III., except that the interval between the clicks was made .0065 sec.

	Lesser Group.			Standard.			Greater Group.		
Subject.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.	No. of Obser.	No. of Errors.	Per Cent. of Errors.
H.	85	10	11.8	85	26	30.5	85	27	31.8
S.	89	22	24.7	93	36	38.7	89	36	40.4
B.	96	5	5.2	96	31	32.3	96	32	33.3
Total.	270	37	13.7	274	93	33.9	270	95	35.2

Total No. of obser., 814; total of errors, 225; percentage of errors, 27.6.

In both tables the subjects show greater percentages of errors upon the greater group than upon the standard or lesser group, and greater upon the standard than on the lesser

group, thus confirming the results of Table II., and showing that the distribution of the errors does not rest on the particular conditions of the earlier experiments. The percentages of errors for the faster rates are on the whole considerably increased.

An attempt was now made to measure the difference thus discovered by increasing the number of clicks in the greater group. The results appear in Table V.

TABLE V.

Giving the number of observations and percentages of errors, when the greater group was made greater by two clicks than the standard, and by three greater than the lesser group. The interval between the clicks was .0065 sec., and between the two groups .25 sec. The standards used were 6, 7 and 8. With 6 were given 5 and 8; with 7 were given 6 and 9, and with 8 were given 7 and 10 as comparison groups.

	Lesser Group.		Standard.		Greater Group.	
Subject.	No. of Obser.	Per Cent. of Errors.	No. of Obser.	Per Cent. of Errors.	No. of Obser.	Per Cent. of Errors.
H.	42	14.2	42	11.	42	4.9
S.	30	23.3	31	35.6	30	20.
B.	42	7.1	42	19.	42	2.3

The effect of the double increase of the greater group was to reduce the percentages of errors upon it below either those on the standard or on the lesser group. The percentages of errors on the standard have also been reduced, as was to be expected, for increasing the difference between the standard group and the greater comparison group renders the mistaking of the standard for the greater less likely as well as the mistaking of the greater for the standard.

The proportion of errors made on the standard is worth a little further consideration. Table II. shows that the percentages upon the standard were always greater than upon the lesser group, and with one subject greater upon the standard than upon the greater group. What the explanation of this difference is, is not easy to say, for the subject knew in each case that the two groups would be alike as often as they were different. Possibly some qualitative difference, subjective or objective, between the first and second groups, lies at the bottom of it. It was impossible to do away absolutely with

all such differences, and impossible also to say in what the differences consisted. This may have led the subjects to judge the second set to be different from the first more often than they should. But it may also have been wholly subjective. When the subjects were expecting a difference, they found great difficulty in not imagining one, even though there was none.

A more interesting question at present is, however, the greater number of errors upon the greater group as compared with the lesser group as shown in Tables II., III. and IV. and the effect on the distribution of the errors produced by the increased difference between the standard and greater group shown in Table V. In this case the qualitative difference just considered cannot be made the basis of judgment. It seems rather to be a question of the behavior of the memory after-image. There was a kind of subjective feeling of disappointment when a lesser number followed the standard, and of superfluity when a greater number followed. Possibly the explanation should run something like this: The intervals here dealt with are very much shorter than any we meet with in daily experience, and for that reason in attempting to recall such an interval, we may imagine it too large; we think of it as corresponding to other experiences. This enlarged image of the first interval is brought into comparison with the sensation produced by the second and thus the second seems very much shorter than the first. Our memory image of the first actually increases in length during the interval between the first and second sets of clicks. When the lesser group follows the standard, the second seems decidedly less than the first; but when the same or a greater number follows, the difference is not so great and more difficulty in discriminating is experienced. The experiments of Table V. show that the amount of lengthening in the memory after-image was more than compensated by the increase of the difference between the standard and the greater group. The apparent increase in the first group for standards such as were used must, therefore, be in general less than the time of two clicks, that is, .013 sec. It was supposed that this apparent increase might be greater for longer times than the .25 sec. that in these experiments separated the groups, but when the interval between two groups was increased to six seconds, though a slight increase in the percentage of errors took place, their distribution remained practically the same. The question of fatigue may also enter here as a possible explanation. To produce successively and rapidly a sensation of given intensity, the stimulus must be constantly increased. In order, then, that the second set of clicks shall produce a sensation equal to the

first, the second set must be greater than the first. When the second set of clicks is less than the standard, the sensation would fall far short of the first. This would seem to explain the feeling of relief which followed so perceptibly on the lesser comparison, but it involves the supposition that the number of clicks or the time they occupy is inferred from the intensity of the general effect that is produced.

Tables III. and IV. have been considered so far in relation to the distribution of the errors only. They bear, however, upon the general question of the investigation. Table III. reinforces Table I. in showing that, for two observers at least, a difference of one in excess or defect can be recognized with a standard group of 8 clicks separated by intervals of .011 sec. Table IV. shows that the same is true when the intervals are reduced nearly one-half, namely, to .0065 sec., though the percentage of errors is somewhat greater. Results obtained from experiments where the subject is given three possibilities of answer are not strictly comparable with those in which only two possibilities are offered, but so long as there is no tendency to confuse the greatest and the smallest of the three stimuli (for example, in the experiments giving these tables, to call a 7-click group a 9-click group, or *vice versa*), the case is not very different from that of the two-possibility experiments. In the experiments giving Tables III. and IV. there were very few answers of this doubly wrong kind, and 75% of right answers may be assumed as the measure of discrimination, as in the experiments giving Table I.

With the apparatus used it was impracticable to attempt a further shortening of the intervals beyond .0065 sec.; accordingly attention was turned to the effect of increasing the number of clicks in the groups, with a view to discovering if possible the limit in number for an interval that was easily at command; in this case .0075 sec. $= 133+$ clicks per sec. Tests were made with groups of 13, 16 and 23 clicks as standards. The results reached are grouped in the following table :

TABLE VI.

Giving the number of observations, the number of errors, and the percentage of errors, when the standard was compared with the same number or with a greater or a less number. The interval between the clicks was .0075, that between the groups .25 sec.

STANDARD 13.

Subject.	LESSER GROUP.			STANDARD GROUP.			GREATER GROUP.		
	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	17	0	0	24	15	62.5	22	11	50
S.	25	11	44	25	13	52	25	11	44
B.	22	3	13.6	21	10	45.7	21	9	23.3

Total number of observations, 202; total of errors, 83; percentage of errors, 41.9.

STANDARD 16.

Subject.	LESSER GROUP.			STANDARD GROUP.			GREATER GROUP.		
	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	32	13	40.6	26	13	50	33	15	45.4
S.	35	9	25.7	39	22	56.4	32	12	37.5
B.	29	10	34.8	28	11	39.2	29	12	41.4

Total number of observations, 283; total of errors, 117; percentage of errors, 41.3.

TABLE VI.—*Continued.*

STANDARD 23.

Subject.	LESSER GROUP.			STANDARD GROUP.			GREATER GROUP.		
	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.	No. of Obser.	No. of Errors.	Per cent. of Errors.
H.	18	11	61.1	21	12	57.1	22	10	45.4
S.	26	15	57.7	28	17	60.6	26	13	50
B.	17	7	41.1	27	18	66.6	21	8	38

Total number of observations, 206; total of errors, 111; percentage of errors, 53.8.

The percentages that the total number of errors are of the whole number of observations on any standard have increased with the increase of the standard. Errors resulting from a confusion of the lesser group with the greater are much more frequent upon 23 as the standard than upon 13 or 16. The percentages of such errors of the whole number of observations are, on 13, 5.4%; on 16, 5.3%; and on 23, 17.4%. The impossibility of judging the difference of one click with a group of 23 when they are separated by .0075 sec., is not to be doubted; only 66.6% of errors would be expected if the answers were dictated wholly by chance. For 13 and 16 the number of right answers is considerably more than could be expected from chance, but considerably less than the required 75%, if we apply the criterion of the two possibility experiments. It would seem then that the ability to recognize a difference of one click under the conditions of these experiments does not extend far beyond groups of 8 or 10 clicks and fails completely at something over 23.

It remains to speak of whether these are time or number judgments. The separate clicks could be apprehended, but counting was out of the question, and it was impossible by any voluntary muscular adjustment to name or record the number. The number of clicks in the group seemed often, if not always, less than the number actually known to be present. The only attempt on my part towards a muscular adjustment was to contract the muscles of the tongue and

larynx twice in rapid succession in such a way as to produce a grating sound when the vocal cords were made to vibrate, thus imitating the groups of clicks to be compared. This took place when I was in doubt. The decision was based upon what seemed to be an equal or an unequal amount of stimulus. An introspective account of how the groups were compared is difficult, because of the extreme concentration of attention required. If the judgment is a number judgment at all, it is something quite different from the number judgments of daily life, possibly akin to the quantitative estimates of groups of objects made by animals and by children before they have learned to count, and, indeed, by adults when for any reason counting is not easy. That time judgments also played a part seems very probable; and the fairest description of the experiments undoubtedly is to say that they are judgments of filled time.

Summary. The experiments that have been described make the following answers to the questions at the beginning of the paper: To question 3 the answer is that the presence of one more or one less click in successive groups can be recognized (probably by the greater length of time required for the larger group) when the number of clicks is not too large and their rate not too rapid. When the rate is 133 per sec., the number cannot very much exceed 10, though with groups of 13 and 16 a difference of one can sometimes be recognized. When the number of clicks is 9 or less the rate may rise at least to 153 per sec., and perhaps higher.

To question 2 the answer is that it is extremely improbable that any clicks are lost in the perception of a rapid group at least up to 153 per sec.; for groups of 8 and 9 can be distinguished at that rate. If the experiments involve a time judgment, it is clear that one one-hundred-and-fifty-third of a second can be recognized, and if so, there would be no need that the images of clicks at that rate should over-lap in consciousness—over-lapping was the cause of loss in Hall and Jastrow's counting experiments. If the judgment was a number judgment, the loss would also seem to be excluded. In some cases where a temporary defect in the instrument caused the actual dropping of a click from the middle of a group, this fact was very easily recognized, though this of course is quite a different thing from the unconscious subjective dropping implied in the question. Particulars as to rates at which this dropping occurred have unfortunately not been preserved, but the statement is believed to be true for all rates.

To question 1 the answer is that for the mere perception of discreteness, number is probably without influence. The fact

that it is less easy to tell 10 clicks from 9 than 5 from 4 shows only that a difference of 1 in 10 is less easy to recognize than a difference of 1 in 5, a result that accords with the psycho-physic law. With reference to the tendency to blend, such a result shows nothing. No greater tendency to blend in the case of the larger numbers was noticed by the subjects. When the question is one of estimating the number of clicks, a slower rate for larger numbers may very well be required.

A few words upon the work of others on related topics will serve to put the results of this study in their proper perspective. Exner found in his studies of reaction-times[1] that his subjects could tell "good" (i. e., quick) reaction-times from slow ones when the difference amounted to about .01 sec., thus seeming to show an ability to perceive a time as small as that amount.

The general form of these experiments is not so very different from those of Dietze's work on the *Umfang* or extent of consciousness,[2] but the intervals used were very much smaller than his, his shortest being .11 sec., or exactly ten times the largest in these experiments. He also found a decided falling off in ability to judge correctly when intervals less than .24 sec. were used, a result different from Mr. Bolton's, but one that may depend either on a difference in experimental conditions, or on a difference in the manner in which groups of clicks of such different rates are judged. Mr. Bolton's subjects found no tendency to such rhythmical sub-grouping of the clicks as Dietze found.

The work of Hall and Jastrow, already mentioned, is somewhat analogous, but different both in the size of the intervals (theirs being .0895 and .0523) and the mental process involved, the effort in their case being generally to count the clicks, or at least to estimate them in numbers. In Mr. Bolton's experiments counting was out of the question. Their observation that filled time seems longer than vacant time could be frequently repeated in these experiments. When, as was natural, attention was concentrated upon the groups of clicks, each group seemed clearly longer than the empty time between them, though with even the most numerous groups the latter was a full third longer. The function of attention in this illusion is certainly a very important one.

Dr. F. Schumann has published two papers on the comparison of small intervals of time; the first in the *Nachr. v. d. Ges. d. Wiss. zu Göttingen*, 1889, No. 20, and the second (a preliminary communication) in the *Zeitsch. für Psychol.* II. 1891, 294–296. Schumann's experiments, however, were made upon empty intervals of time, and though short, did not approach in brevity those of Mr. Bolton, his shortest reported being .15 sec. The same experimenter has repeated in part the work of Dietze with practically the same numerical results, but with a totally different conclusion as to their meaning.[3] Schumann believes that when two such groups are compared the process is somewhat as follows: Each member of the first group as it arrives is registered in memory (in some cases this is clearly done by an accompanying muscular contraction). When the second group is received there is a tendency for the first to be

[1] Pflüger's Archiv, VI. 1873, p. 613—. But see also an explanation of Wundt's (Physiol. Psychol. 3rd Ed. II. 238), which rests upon other than a time discrimination.
[2] Wundt: Physiol. Psychol. 3rd Ed. II. 248–252. Dietze, Philos. Studien II.
[3] Zeitschrift für Psychologie, I. 1890, 75–80.

reproduced in its original number. If the second group contains more clicks than the first, the tendency to reproduction ceases before the cessation of the actual clicks of the second group; if the second contains a less number, the tendency over-runs; if the same number, both tendency and clicks cease together. On these inner experiences the judgment of the subject rests. The "feeling of too much or too little" that Mr. Bolton's subjects speak of, might well point to something of this kind. Its origin, however, could hardly in this case be a muscular response to each click, unless, perhaps, the tympanic muscles respond reflexly, for the most rapid voluntary movements are very much slower than the slowest clicks used in his experiments. If the tendency to reproduce a group of clicks in its exact number is a function of the sensory parts of the cerebrum, there would seem to be no difficulty in explaining the judgment of even these very rapid groups on Schumann's hypothesis.

E. C. S.

STATISTICS OF DREAMS.

By Mary Whiton Calkins.[1]
Instructor in Psychology, Wellesley College.

The phenomenon of dreaming has rarely been discussed or investigated in a thorough and in an experimental manner; of description, of theory, of discussion, of poetical analogy and illustration there has been no end; of accurate observation almost nothing.[2] The most scientific books—those of Maury and of Tissié—have been wholly or chiefly the result of the observation of abnormal subjects, and in the interest, more or less distinctly, of pathology. The fullest discussion of the subject—the works of *Radestock* and of *Spitta*—are largely compilations of the recorded dreams of other people.

The basis of the following paper is the accurate record of the dreams of two people, from notes made by themselves during the night, and supplemented by careful study and recollection on the following day. The investigation was undertaken at the suggestion of Dr. Sanford and was carried on for six or eight weeks. Its method was very simple: to record each night, immediately after waking from a dream, every remembered feature of it. For this purpose, paper, pencil, candle and matches were placed close at hand. Early on the next day, with rare exceptions, these abbreviated notes were re-read, copied in full and enlarged by comments, by description of all attendant circumstances, and by the indication in all possible cases of the connection of the dream with the waking life. During the first weeks of the experiments, an alarm clock was used to wake the experimenter at different hours of the night. Later, the use of the clock was discontinued because the excitement of waking through its agency was often so great as to prevent connected memory of the dream.

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[1] Received as a private pupil, 1890-91.

[2] Of this character, I know only the papers of Prof. Nelson (Am. Journ. Psych., I., p. 385); Heerwagen (Wundt's Philosophische Studien, V. p. 88); Ives Delage, Revue Scientifique, July, 1891.

Omitting, so far as possible, all general discussion of the nature and the explanation—physiological and psychological—of dreaming, I shall chiefly discuss the classified results of the observations to which allusion has been made. It will be well, therefore, to give at once the general results of the statistics and to indicate some of the difficulties. Chief among these is that of recalling dreams after one has waked. Doubtless this is due, both to the lack of congruity between the waking and the dreaming life, and to the slight excitement which often accompanies the act of waking. To recall a dream requires usually extreme and immediate attention to the content of the dream. Sometimes the slight movement of reaching for paper and pencil or of lighting one's candle seems to dissipate the dream-memory, and one is left with the tantalizing consciousness of having lived through an interesting dream-experience of which one has not the faintest memory. To delay until morning the record of a dream, so vivid that one feels sure of remembering it, is usually a fatal error. During the progress of the observations, the account of one dream, apparently of peculiar significance, was written out in the dark by the experimenter, who then sank off to eep with the peaceful consciousness of a scientific duty well done. In the morning the discovery was made that an unsharpened pencil had been used, and the experimenter was left with a blank sheet of paper and no remotest memory of the dream, so carefully recalled after dreaming it.

The difficulty in remembering dreams suggests, of course, the impossibility of an exhaustive enumeration of their peculiarities and of any positive conclusions from the figures of such tabular views as will be offered. At the best, one may discuss only dreams as remembered, and the power of recollection varies widely with age, temperament, health and other conditions. A dream which is remembered in detail must have been a vivid one, but it is not true, as we often assume, that a vivid dream is necessarily well remembered, for both dream-records here considered contain several cases of "vivid dreams immediately forgotten." In one case a distinct memory of a vivid visual dream of a newspaper included no faintest recollection of one word on the page. It seems certain, therefore, that most people dream much oftener than they think.

While some dreams thus escape the memory, the very effort to record may well tend in the contrary direction, toward an increase of their number. The observer with the recording "on his mind" may sleep less soundly, dream more and wake oftener. Too great an anxiety certainly has this effect. Finally, the student of dreams is

in danger of reading into his dream much that is characteristic of the waking consciousness, something as one interprets an animal's actions by one's own. As the dog is said to "reason" when he performs—instinctively, for aught we know—an act which would imply reasoning on our part, so emotions, thought and deliberation, which were really absent from a train of dream-images, may be supplied by the reflective after-consciousness.

With these preliminary cautions one may turn to the study of the figures. The observers will be designated as S. and C. S., a man of thirty-two, took notes for 46 nights and recorded 170 dreams; C., the writer, a woman of twenty-eight, took notes on 55 nights and recorded 205 dreams, an average of nearly four dreams a night in each case. Neither observer had previously regarded himself as a frequent dreamer. After the records were completed, each worked up the figures for his own dreams, with occasional conferences with the other. Such individual treatment was unavoidable where the relations of the content of the dreams to previous experience were to be considered; some differences were thus introduced into parallel tables, but none of them are regarded as of great importance.

Time of Dreams.

Table I.

Observer S. Number of Dreams Included in the Table, 118.[1]

10.30 P.M.—11.30 P.M.	11.31 P.M.—5.30 A.M.	5.31 A.M.—8.30 A.M.
4 3.4%	52 44%	62 52.6%

Table II.

Observer C. Number of Dreams Included, 183.[2]

Before 4 A.M.			After 4 A.M.	
On Fal'g Asleep Eve'g.	11 P.M.-2.30 A.M.	2.31 A.M.—4 A.M.	4 A.M.—6 A.M.	6.01 A.M.—8 A.M.
1	22	26	78	56
49 (26.8%)			134 (73.2%)	

[1] The records on the remaining 52 dreams did not specify the time with sufficient exactness for use in this table.

[2] The total number of dreams recorded was 205. The last 7 dreams recorded together with 15 others have been omitted from this count, because no sufficient record was made of the time at which they occurred.

The general result of these tables is, of course, to confirm the opinion that most of our dreams occur during light, morning sleep. They show, however, conclusively that the sleep of the middle of the night is in no sense a dreamless sleep, for, of course, the recorded number of "before 4 A. M." and "before 5.30 A. M." dreams is only a record of remembered dreams, and after the use of the alarm clock had been discontinued, fewer dreams from the middle of the night were recorded because of the infrequency in waking.

The inquiry whose results are next tabulated was undertaken in order to discover the effect, if any, of the time of a dream, on its connection with the previous waking-life.

TIME VERSUS SUGGESTED CHARACTER OF DREAMS.

TABLE III.

Observer C. 194 Dreams Included.[1]

	Before 4 A.M.			After 4 A.M.		Time Undet'd.	Totals.
	Falling Asleep	11–2.30	2.31–4	4.01–6	6.01–8		
No Suggestion Traced.		1	4	11	5	1	22 (11.3%)
Slight or Vague Suggestion or Mere Congruity.		8	11	33	22	6	80 (41.3%)
Suggestion.	1	7	8	26	17	5	64 (33.%)
Close Suggestion.		6	2	7	10	3	28 (14.4%)
Totals.	1	22	25	77	54	15	194
	= 48			= 131			

[1] The total (194) differs from that of Table II., because 4 dreams, occasioned by peripheral sensation, have been rejected from the count, and 15 of undetermined time included.

The most obvious conclusion from this table is the close connection between the dream-life and the waking-life. In only 11 per cent. of the dreams is it impossible to discover such connection. When, however, we seek the answer to our immediate question, it seems impossible to calculate accurately the influence of the time of a dream upon the degree to which it is associated with the waking experience. It is impossible to conclude that the dreams which follow most immediately upon our daytime experience will be more closely united with it than the morning dreams; and equally wrong to conclude that dreams during the lighter sleep of morning, nearer the waking-state, will be more congruous with the waking-life.

TABLE IV.

Observer S. 170 Dreams Included.

TIME.	10.30—11.30	11.31—5.30	5.31—8.30	UNDETERMINED	TOTAL.
No Suggestion Traced.	1	29	34	25	89 (52%)
Vague Suggestion or Mere Congruity.	1	13	21	16	51 (30%)
Suggestion Traced.	2	10	7	11	30 (18%)
Total.	4	52	62	52	170

The figures in Table IV. are of the same general import as those of Table III., though the proportion of dreams suggested by waking-life is much smaller. This is undoubtedly due in part to a different criterion in classifying. The dreams of S. were very frequently pervaded with an atmosphere of experiment, not definite enough to warrant placing them in the class of vague suggestion, but undoubtedly dictated by the waking occupation, though possibly also by the very attempt to record the dreams. Furthermore, dreams in which the companions of waking-life were present were not included as suggested dreams unless some incident of the dream was also suggested.

The next tables embody an attempt to classify the dreams according to their vividness. The difficulty of this classification has been already suggested. Vividness and the fact of

being distinctly recalled are not equivalent characteristics. Because a dream is indistinctly remembered, it is not fair to conclude that it is not a vivid one, yet it is not safe to trust altogether to a mere memory of vividness without any support for the impression. The degree to which a dream is recalled (which may be roughly gauged by the length of the written record) is thus a convenient, though not an absolute, test of the vividness of a dream.[1] Both factors enter into the following classification: Class I. includes dreams which were very vivid and were remembered in detail; class II. comprises vivid dreams, usually less vivid than those of the first class, and less accurately remembered; class III. includes, in general, dreams of still less vividness which are recalled in less detail, but contains a few cases of dreams which are largely forgotten, though remembered as vivid;[2] finally, class IV. contains the record of dreams largely forgotten, which—so far as recalled—were indistinct.

VIVIDNESS OF DREAMS.

TABLE V.[3]

Observer S. Number of Dreams Included, 170.

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
No. Cases.	40 (23.5%)	41 (24.1%)	44 (25.9%)	45 (26.5%)	170
Total No. of Lines.*	1062	331	128	96	1617
Average No. of Lines.	26.5	8.1	2.9	2.1	9.5+

* Average number of words in a line of record, 12.2.

[1] The plan of estimate by the length of the record is borrowed from Prof. Nelson. Am. J. Psych. I. p. 383.

[2] For instance, the newspaper dream mentioned above.

[3] So nearly as I can judge, vividness, as opposed to length of record, was a little more closely the standard of classification in Table VI. Notice the greater length of dreams in Table V., except under class III.

TABLE VI.

Observer C. Number of Dreams Included, 191.[1]

	Class I.	Class II.	Class III.	Class IV.	Total.
No. Cases.	32 (16.8%)	65 (34%)	57 (29.8%)	37 (19.4%)	191
Total No. Lines of Record.*	705	443	174	66	1388
Average No. of Lines.	22	6.8 (circa)	3	1.75 (circa)	7.25 (circa)

* Average number of words in a line, 8.6 (circa).

In general, the dreams of C. were written out in rather less detail than those of S., so that the comparative length of the records does not give an accurate idea of the comparative length of the dreams of the two observers. A notion of the principle of classification may be gained by referring to dreams 43, 193 (C.); 28, 111 (S.),[2] which have been included in class I.; to dreams 91 and 94 (C.),[3] which belong to class II., and to dreams 19 and 68 (C.),[4] of class III.

These results are perhaps fairly indicative of the average vividness of dreams. The following table, and the inference which may be drawn from it, suggests that vivid dreams are by no means confined to morning hours, as might perhaps be supposed:

[1] Fourteen dreams, in which the record is inadequate, are omitted.
[2] Pages 336, 328, 325, 324. Observe that only dream 43 is quoted in full.
[3] Pages 326 and 325. See also dreams of S. in foot note, p. 132.
[4] Pages 343 and 336.

DREAMS BEFORE AND AFTER 4 A. M.

TABLE VII.

Observer C. Number of Dreams Included, 184.[1]

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
Before 4 A. M.	2 (4.26%)	10(21.28%)	20(42.55%)	15 (31.91%)	47
After 4 A. M.	27 (19.71%)	54(39.41%)	33(24.09%)	23 (16.79%)	137
	29 (15.76%)	64(34.78%)	53(28.81%)	38 (20.65%s	184

DREAMS BEFORE AND AFTER 4 A. M.

TABLE VIII.

Observer S. Number of Dreams Included, 109.[2]

	CLASS I.	CLASS II.	CLASS III.	CLASS IV.	TOTAL.
Before 4 A. M.	5 (15.6%)	9 (28.1%)	8 (25%)	10 (31.3%)	32
After 4 A. M.	24 (31%)	25 (32.5%)	15 (19.5%)	23 (16.9%)	77
	29 (26.6%)	34 (31.2%)	23 (21.1%)	38 (21.1%)	109

It will be observed that as large a proportion of dreams of a moderate degree of vividness (Classes II. and III.) occur during the night as toward morning. The percentage of most vivid dreams is greater after 4 A. M.; that of the least vivid dreams, on the contrary, is greater among the night dreams

[1] The dreams omitted from the count in Table VII. are the first fourteen, of which the record is not an adequate one; and the last seven, in which the time recorded was not kept.

[2] The dreams omitted are 43, whose time is not recorded; and 17, whose time (as between 11.30 and 5.30) cannot be classified on this plan.

than among those of the morning. We conclude, therefore, that the dreams after four o'clock tend to be more vivid than the earlier ones, but that the distinction is no absolute one.

Dreams may be most conveniently classified according as they are connected through sense excitation with the immediate present, or through the fact of association with the waking life of the past, according, that is, as they contain presentations, or merely representations. Or, in physiological terms, dreams are occasioned or accompanied by peripheral or by merely cerebral excitation. When I dreamed of hearing fire-bells, and then seeing from my window the burning of a church, and waked to find my window-shade flapping back and forth in a strong wind, this was evidently a presentation dream; on the other hand, when I dreamed of meeting Caroline Schlegel, after actually reading a story of her life, it was a representation dream. But as Spitta (who makes a similar division into *Nervenreizträume* and *Psychische Träume*) suggests[1] no dream belongs exclusively to one class or to the other, so that it is more accurate to refer to the presentation elements and to the representation elements in a dream.

The following are the classified results of the dream-records under this head:

PRESENTATION ELEMENTS IN DREAMS.

TABLE IX.

Number of Dreams Included: For S., 170; for C., 165.[2]

	Vis'l.		Aud.		Tact.		Temp.		Gust.		Organ.		Total.	
	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?	Clear	?
S.	1	1	3	9	1	2		4				1	5	17
C.	1	1	2		1	1	1	1	2		1		8	3

Totals, S., 22=13.2%.
C., 11= 6.7%.

The most striking outcome of the figures is the relatively small number of dreams which can be shown to include any sense-perception. A closer inspection shows that, with both observers, the auditory dreams are of chief importance, a fact

[1] Spitta, p. 213.
[2] Dreams of Class IV. were not considered in C.'s Statistics.

of common experience and readily explained since, next to sight, hearing is the sense most constantly exercised, and of the two is the more easily stimulated when the eyes are closed in sleep. Visual presentation dreams are less frequent, but dreams from dermal excitation occur comparatively often, and the occasioning sensation is apt to be much exaggerated. For instance, a dream of struggling to crawl from an elevator, through an absurdly small opening, into an eighth-floor apartment, was traced directly to a cramped position and to a heavy covering across the face; a dream of a sleigh-ride on an intensely cold day, and of hearing that the cornea of a friend's eyes had broken from the cold, was evidently occasioned by a stiff breeze blowing in at the window. Nightmares are often attributed to this cause, as the German name *Alpendruck* shows.

Dreams through stimulus of the lesser senses, smell and taste, are infrequent, but not unknown. Our observed dreams include none through stimulus of the sense of smell, but there are two clear instances of the taste-dream, though in most so-called taste or smell-dreams one dreams of seeing food or flowers, not of actually tasting or smelling.

Of dreams whose starting point is organic sensation[1], there are only two cases among those classified in Table IX. Common instances of such dreams are fright dreams in their simple form, or in the exaggerated nightmare stage,[2] which are largely due to organic sensations of pressure for breath, of quick heart-beating, or of digestive discomfort. Our records contain no instances of dreams initiated by actual movement.

So nearly as I can judge, the intrusion of the sense-impression into the dream-train is during the transition between sleep and waking, and the dreams or parts of dreams preceding the waking dreams are those undisturbed by sense. In any case, the emphasis, by most writers,[3] of dreams of this nature seems to be due to the fact that the dreams from which conclusions have been drawn are, in almost all cases, particularly striking and unusual dreams, and therefore not fairly representative of the warp and woof of dream experience. The most important feature of the present investigation is really the very prosaic and ordinary nature of most of the dreams recorded and the exhaustiveness with which, during a number of weeks, all remembered dreams, and not merely peculiar and abnormal ones, were registered.

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[1] For a fanciful account cf. Scherner.
[2] Cf. Radestock, *op. cit.*; and Kant, Anthropology, § 34, p. 105.
[3] *E. g.*, Sully, Illusions.

The representation elements of a dream may enter through association of the first stage of the dream with the waking life, in which case we have what is properly a cerebral or representation dream, or may enter through association within the dream of one element with another. In this latter sense, all dreams contain representation elements, for the very transformation of the sense-excitation of the presentation dream into an image requires the associative process.

REPRESENTATION DREAMS.
TABLE X.

	Representation Dreams.	Presentation Dreams.	Undetermined.	Totals.
S.	148[1]	22		170
C.	191	11	3	205

The following table presents a classification of what we have called representation elements of dreams:—

REPRESENTATION ELEMENTS[2] IN DREAMS.
TABLE XI.

	Representation of Cognition.						Representation of Action.	
	Vis'l.	Audit.	Derm.	Gust.	Olf.	Gen'l.	Vocal.	Other.
S.	113	76	7	0	2	1	36	44
C.	127	81	14	0	2		71	71

Total number of dreams considered: S., 133; C., 165.[3]

The preponderance of visual images is very striking. The relation to auditory images (the figures indicate in both instances a ratio of about three to two) seems to me the more

[1] All which cannot be counted as presentation dreams have been counted as representation dreams.

[2] The table is not a classification of dreams, but of representation elements. Representations of more than one sense and of action often occur in the same dream. Hence there can be no attempt to make totals correspond.

[3] All dreams of class IV. have been omitted from the total 205 dreams because too imperfectly remembered, with a few others not classed as IV., but incompletely described (in all, 40).

remarkable in the case of C., who has no very vivid, visual imagination, but on the other hand thinks habitually with words rather than with concrete images.

Next to visual-dreams, word-dreams have been by far the most frequent, in the case of C., and have occurred often with S. There are many cases, in the former records, in which the words have seemed to suggest the dream or to form its significant part. The words have been spoken, heard, read and sometimes written; very often they have been spoken and heard, or read and written, or spoken and read, in the same dream. The following table summarizes these results:

WORD ELEMENTS[1] OF DREAMS.

TABLE XII.

	Spoken.	Heard.	Visibly Read.	Written.	
				Clear	?
S.	36	63	10	4	2
C.	73	78	11	2	

Total number of dreams in which words occur: S., 77 (of 147);[2] C., 110 (of 165).[2]

English was the language of all these dreams, but in the following cases foreign languages were represented in the dreams of C., usually only by a word or a phrase, but sometimes more at length: German, 5 cases; French, 5 cases; Italian, 1; Latin, 1.

C. had many dreams in which the connection with waking life and the occasion of the whole dream or of some part of it was a merely verbal association. Some of these dreams consisted wholly of the philosophical expressions, more or less logically combined, of the authors whom C. was then very intently studying. In one case the application of such a philosophical phrase was very oddly turned. C. had been reading Fichte's *Wissenschaftslehre*, and the day before had wrestled with his description of the non ego as "*bestimmbar aber nicht bestimmt.*" In the dream, C. was discussing with a friend a matter of personal expenditure and told him, quite properly, that it was "*bestimmbar aber nicht*

[1] Many dreams contain both spoken and heard words, etc.

[2] Dreams of class IV. not included. 14 dreams included, which were omitted from Tables VI. and VII. because time-record is not given.

bestimmt." Another time, after reading in the evening that Elizabeth Phelps Ward had bought a farm at Oak Hill, a dream followed of lunching at the Oak Farm restaurant.

Sometimes a word was visualized in a dream. C., for instance, after reading just before falling asleep a list of words of which one was "snake," dreamed of walking through tall grass and of seeing a snake in the path, and after reading Everett's Fichte, dreamed of meeting Dean Everett. Occasionally the dream began as a verbal one and grew from word to act, as when from the reader of a romance, the dreamer became one of its actors. The following table covers most of these cases.

TABLE XIII.

Observer, C. 28 cases included.

Words suggest dreams in 28 cases.	
Words are transferred in 17 cases.	
With same application, 14 cases.	
Philosophical application: clear, 8; doubtful, 4.	
Other application, 2.	
With changed application, 3.	
Philosophical application, 1.	
Other, 2.	
Words are visualized in 3 cases.	
Words enter otherwise, 8 cases.	

Words are exactly remembered in nineteen cases by S., in five only, by C. Three of these are cases of puns, which would never have been perpetrated in the waking life.

II.

An important result of the study of dream phenomena is the demonstration of the essential congruity and continuity of the dream life with the waking life. The stages and aspects of psychic activity which make up our dreams are recognized as those of which our whole conscious life consists, and a careful examination of dream phenomena tends to establish what Spitta calls the "complete unity of the human mind."[1]

Of course perception occurs only in "presentation dreams," in which actual sensation has stimulated the dream activity, and the characteristic sort of dream-consciousness is the imaginative. A dream is in fact most simply defined as an illusion of the imagination in which images are reproduced, accurately and completely, or in fresh and varying combination. C. twice dreamed in exact detail some immediately preceding

[1] "Völlige Einheit der menschlichen Seele."

event, a case of the simplest sort of mechanical imagination,[1] in which the association was that of total recall.[2] Far more often the representation of parts of one experience is combined with the representation of congruous parts of another, and most often the imagination reaches the emotional and erratic plane of fancy.

But besides this unrecognized imagination, which is the dream activity as such, there is overt imagination, just as there is memory, within a dream. In illustration of this, compare the following dream record—an extract from a longer one.[3]

April 7–8, 6.50 A. M. Observer S., Dream 111. "Before that, had been camping out with a lot of '83 people and others that I did not know, not far from where the gypsies used to camp on Lake M. - - - - -. I think I said I would stay as long as anybody at the camp, and wondered how it would seem at last when everybody had gone, and if it would not then seem good to get back to a boarding-house. (I thought of the one to which I should go in O., and also thought how things would have to be raked up about the camp, in the dust)". The parenthesis contains the record of a distinct case of imagination within a dream.

Accurate memory of real events in the waking life is not frequent, but occurs in well-marked cases. For instance, C. twice recalled incidents appropriate to the dream situation, of travel in Italy four years before; in another case, two factory-girls applied for help in getting work, and C. remembered the circumstances of certain actual labor troubles in Lynn, which made it unwise to send them there.

The recorded dreams abound in instances of false memory, paramnesia. Dream persons and dream places are remembered, on waking, to have been widely different from the real people and places. Or, the dream consciousness accounts for the presence of some unexpected person, for some unfamiliar detail of the surroundings, by "remembering" something which never really happened.

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[1] It is misleading to call this, as Maury does, "souvenir ignoré," or "mémoire . . . non consciente." Memory is distinguished from imagination in that it involves conscious reference to the past and to the self.

[2] Cf. James, Psych., I., p. 588.

[3] All the dreams quoted from the records have been copied exactly, except for amendments of the English, not affecting the sense. Dashes (- - -) indicate omissions, by the editor, of matter irrelevant to the case in hand; asterisks (* * *) are copies of those *Gedankenstriche*, made in the original records.

Not only imagination, but real thought occurs in dreams, though the fact is often denied.[1] The frequency of conversation in dreams might be looked upon as a proof of the existence of the lower stages, conception and judgment, of thought-activity, for both are necessary to the use and combination of words. But such dreams may be instances of merely visual or auditory memory of words, and if there were no instances of explicit judgment and of reasoning in dreams, the presence of thought could hardly be proved. But not only do we form judgments in our dreams, sometimes correctly, though often inaccurately, but we carry on whole trains of reasoning, reasoning sometimes incorrectly, often correctly from absurd premises; and less often both correctly and from natural premises. (Cf. the dream quoted on page 328.) From many illustrations in the dream-records, I select the following:—

Incorrect Reasoning. Tuesday, March 31, 6.50 A. M. Dream 94. Observer C. "When we think substantiality and causality we think that which is out of time—but time we know exists, therefore substantiality and causality must be given up. This is Mr. Huxley's argument."[2]

[1] Cf. Spitta's dogmatic statement, p. 127 (see also p. 139); "Die traumende Seele ist unfähig richtig zu schliessen."

[2] An instance of correct and ingenious reasoning in the dream of S. I quote because the circumstances are so extraordinarily improbable, though its very elaborateness lays it under a certain suspicion of unconscious editing. March 15-16. Time, undetermined. Dream 28. (Observer S.) "In the sitting-room of the 13th street house. L. [a cousin] is there with her baby, and about to fit him with a new skull. The skull is a great deal too large for such a little baby (it was one which had belonged to some relative of the child, that had died sometime before; the skull was white and nicely cleaned like an anatomical specimen). I reflected that this was probably frequently done, and for this reason one sees so few skulls of children—they are transplanted to the heads of other children. I remembered that Dr. Z. had only one - -. I disjointed the skull at the sutures, and it came apart nicely. L. took the parts and went with the baby and some one else out of the room to perform the operation. I wondered at their coolness in prospect of it. Presently they returned with the baby. The child seemed a little upset, but not in a collapse, and only showed a partially healed wound over the top of the head from side to side where the scalp had been cut. They were not even going to keep the child indoors, but gave him to me to carry out. I set off with him toward the high school, and after a little found that he could talk, though making some errors, as if not entirely used to talking. As we continued he talked more and grew considerably (though I didn't notice it at the time, and only now remember that he was a good deal bigger on the return than on setting out). The theory seemed to be that by getting an older skull he came into, or rapidly adapted himself, to the size and attainments of the previous owner of the skull. I was struck with the fact that this put the active and organizing principle—the soul—in the skull instead of the brain—and naturally was filled with wonder."

The following table includes these cases of imagination, of memory and of thought in dreams. So constant an effort has been made not to supplement the dream facts, by waking experiences, that the figures are rather too small than too large:—

MEMORY, IMAGINATION AND THOUGHT IN DREAMS.
TABLE XIV.

Number of Dreams Included: S., 133; C., 165.

	MEMORY.			EXPLICIT IMAGINATION.		THOUGHT.	
	Accurate.	Paramnesia.					
		Clear.	?	Clear.	?	Clear.	?
S.	12	27	6	10	1	23	12
C.	12	23	2	7	4	19	10

Dream 94, quoted on page 325, from C.'s dreams, is one of twenty-four called "philosophical." The record of the shorter dreams among them suggests at once that they were really not in the least philosophical or reflective in character, but mere parrot like memory of the words met over and over again in reading. I am inclined to the view that this is often the case, but a dream like "Mr. Huxley's argument," quoted above, is evidently one of thought, however inaccurate, on a metaphysical subject. Another case of the same sort is Dream 91, March 31, 6.50 A. M. (Observer C.). "Father is arguing with certain people, especially with Mr. S. One point emphasized is causality. I like father's general line of thought, but object to his view on this point. I say, 'May I speak on this subject?' Father is doubtful if there is time. I begin, 'What means a law of thought?'

Mr. S.— It means nothing.

I— But it stands for a fact. What fact? Causality is a way in which the mind acts * * * * *.

A lady present compliments father on his presentation [of the subject]."

The so-called philosophical dreams of C. have been classified as follows :—

PHILOSOPHICAL DREAMS.

TABLE XV.

Observer, C.

THE PHILOSOPH. CHARACTER MAY BE SIMPLY A VERBAL MEMORY. 16.		THE DREAM INVOLVES THOUGHT. 8.	
Clear.	?	Clear.	?
11	5	6	2

The emotional quality of dreams varies greatly with different persons and with different conditions. C.'s dreams, though they are seldom actually painful, are apt to be pervaded by a generally unpleasant feeling, often rising of course to the stage of positive and excessive discomfort. To certain friends, however, dreaming is an almost uniformly pleasant experience.

The following table gives the meagre results of the attempt to define the prevailing emotions of the recorded dreams :—

EMOTIONS IN DREAMS.

TABLE XVI.

		S.	C.
Pleasure,	Clear,	2[1]	3
	Doubtful,	—	4
Neutral—Surprise and Excitement,		3	2
Pain,	Fear and Repulsion,	2	6
	Shame and Mortification,	3[2]	11
	Disappointment and Regret,	—	9
	Discomfort,	2	5
	Anger,	4	5
	Remorse,	1	—
	Jealousy,	1	—
	Perplexity and Hurry,	—	9
	Uncertain,	3	6
	Totals,	20	60

Indications of volition in dreams are rarer, yet each dream record includes a few cases of well marked deliberation followed by decision, so that one may safely deny the assertion

[1] Includes one of class IV.

[2] Includes one counted before, so not added to total.

often made that the will is never active in dreams. In the following dream there was distinct consciousness of an effort of will :—

Thursday, May 7, 5.30 A. M., Dream 193. (Observer C.) "I am teaching a Greek class, a freshman college class, yet it appears to be beginning Greek. It suddenly occurs to me that they can better and more easily learn the Ionic dialect in connection with the Attic. So I tell them to open their Hadley and Allen grammars at the proper point, and say: 'Now, if you are not in my class next year, how surprised your teacher will be at what you know about Herodotus.' . . . . At this point, a strange woman enters with some one (whom I do not know, but who appears to have some connection with the college). She begins to address the class apparently for some philanthropic object. I step forward and say, 'I am the instructor of this class. Will you show your authorization for interrupting it?' She answers that she has had permission, and finally shows me a certificate dated 1882, and signed, A. E. Freeman. I tell her that the date is too early to be satisfactory. She says: 'But this permission was a renewed one,' and shows me an earlier one. I tell her that the permission is insufficient; but she begins to speak. I interrupt her and say, 'Now one of three things will occur: I shall interrupt you, or I shall get some one to stop you, or you will stop of your own accord.' The woman argues with me; she is poor and needs money. [Note, added later: I am greatly distressed and divided between my wish to be generous and my conviction of the impropriety of her proceeding]. I say, 'No matter; you should have the president's permission' - - - She finally goes."

The study of association in dreams is particularly interesting, though very elusive. One may notice not only the association of the dream with the waking life, but that of the elements of a dream with each other. Its chief characteristic is the spontaneity resulting from the lack of fixed attention in dreaming, and from the absence of standards and norms of objective truth. This spontaneity, however, does not necessarily interfere with the consistency of plot, so to speak, in a large number of our dreams, which preserve one or all of the unities of time, place or interest, so that they may be called by Sully's term, "dramatic dreams."[1]

The spontaneity and freedom of dream-association are due, as we have said, to the general lack of sense-impressions. Sometimes, however, the associative train is sharply turned

[1] Illusions, pp. 172-177, especially p. 175.

by the intrusion of presentations,[1] not recognized as such, or by irrelevant associations, to which insufficient selective judgment is opposed: there result what Spitta calls the "*Ideensprunge*" of dreams. The frequent breaks in the continuity of dream-life make it impossible to classify dream associations after any of the ordinary methods. Often, however, they are emotional. The persistence of some feeling of the waking life seems to determine the character of the associated dream features. From this source arise the dreams of presentiment, according either with hope or with fear.

Two quantitative distinctions of association are constantly illustrated in dreams. We often[2] re-live in dreams, in exact detail, scenes through which we have just passed. More often, as we have said, one very small element of a dream-image is accentuated, and followed by an associated image of which the greater part in turn fades away, leaving a small portion which is followed by its congruent image. In the first case, we have instances of total redintegration. The second illustrate modulation in association,[3] continued focalized redintegration.[4] It is needless to give examples, for everybody recognizes this quality of his own dreams. All the absurdity or incoherence of dreams is accounted for by this feature of the associative process.

The definite question of the subject matter of our dreams is partly answered through our dream-statistics about the persons, places, time and character of these dreams. The lists of *dramatis personæ* are very long, and suggest that the dream world is well peopled.

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[1] Spitta calls attention to the analogous waking experience "Das Selbstbewusstsein findet sich selbst bei allseitigen Wachsein in einem ununterbrochenen Kampf mit der äusseren und inneren Sinnenwelt."

[2] C.'s dreams include two such cases.

[3] Dr. Scripture's term.

[4] James, Psych., I., p. 578.

TABLE XVII.

Observer S.

Number of times of dreaming about people.			
Family, (3)— (A 14, B 8, C 1)—23.			
Professional associates, (20)—56.			
Friends,[1] (69)	In W.,	44	
	B.,	8	
	H.,	4	
	C.,	20	
	Others,	18	
			94
Unknown,			55
		Total,	228 instances.

TABLE XVIII.

Observer C.

Number of times of dreaming about people.			
Family, (5)—A 25, B 7, C 11, D 7, E 3—53.			
Professional associates, (4)—			18.
Friends,[2] (140)	S.,	31	
	In N.,	137	
	" W.,	22	
	" C.,	3	
	" B.,	12	
	Others,	21	
			226
Unknown,			63
		Total,	360 instances.

In the following tables the letters have the same signification as in those last presented :—

[1] Of the letters under this head: W. refers to the present residence; B. to immediately preceding place of residence; H. to a place where two years were spent after leaving college; C. to the place where the childhood and greater part of the observer's life have been passed. The numbers in parentheses refer to the number of different people.

[2] The numbers after the headings refer (as above) to the number of different people. Of the letters under the head "friends," S. refers to one very intimate friend, very often seen, who figured more frequently than any other one person; N. refers to the present residence; W. to the institution in which the observer has been instructor; C. to college town; B. to the home in childhood.

LOCALE OF DREAMS.

TABLE XIX.

Observer S.		Observer C.	
W. and vicinity,	44	N. and vicinity,	78
B. " "	4	W. " "	7
H. " "	3	B. " "	9
C. " "	21	Other places,	11
Other places,	4		105
	76	Place unknown,	39
Place unknown,	57	No place,	21
	133 instances.[1]		165[1]

By far the most conspicuous feature of this place classification is the prominence of places associated with the life of the present. This tallies exactly with the result of the dream census of persons, by which the associates of our dreams are seen to be in large number the companions of the every day life of the immediate present; and the result falls in also with the surprising conclusion from the attempt to classify dreams according to the time in which, as one dreams, one supposes oneself to be living. In all the 375 cases which our records include, there is no instance of a dream in any other than the present time. When the dream was of the childhood's home, or of some person who had not been seen for many years, the apparent age of the dreamer was never lessened to avoid anachronism. Whatever the place or character, the subject was always at his proper age and in his general condition of life (excepting, of course, in the few cases of changed identity).

It is thus evident that the dream is connected with the waking life, and—in the experience of these observers—usually with the recent life. But what besides recency determines the exact line of association, the particular elements which, in a sense,[2] are reproduced in the dream? If we were discussing waking association, we should name two other factors, vividness and frequency, and should especially emphasize the fact that the subjects of conscious thought or imagination are those in which we are most deeply interested. The first impression is that the same holds true of dreams, and that the subjects of dreams are those in which our waking interest is centered. The opinion seems to be strengthened by the fact

[1] Dreams of class IV. are omitted, though they include some cases. Of C.'s dreams, two are omitted from class N., because they belong also to class "B" and to class "Others."

[2] Not a strictly philosophical sense.

of the prominence in dreams of well-known people and places yet a careful examination of one's dreams contradicts this view, as is suggested in the following table:—

CONNECTION OF THE DREAM WITH LIFE.
TABLE XX.

	PERSONAL.		PROFESSIONAL.		TRIVIAL.	
	Gen. char.	Elements.[1]	Gen. Char.	Elements.	Gen Char.	Elements.
C.	57		44	82	104	161
S.	29		50		86	

This table is a very inadequate one, because of the difficulty of defining closely the terms, especially "personal" and "trivial." The former is used, in general, of dreams which are individual in their character, essentially connected with oneself, so that they could hardly be dreamed with reference to another person. Such are mostly dreams about one's own home. The trivial or accidental dream or dream-element is one so unimportant that it might equally well be dreamed with reference to another person or situation.[2]

The figures, however little one emphasizes their rigid accuracy, certainly indicate, what any one may verify by examination of his own dreams, the constancy of the trivial and the accidental, the tendency of the unimportant events of the waking life to reappear in our dreams, and the surprising fewness of the cases in which the dream is associated with that which is of paramount significance in one's waking experience. The so-called "personal" dreams seldom, if ever, refer to really important phases of personal relation.

A little reflection on the subjects about which one does not dream cannot fail to strengthen this view. In times of be-

[1] "Elements" means "parts of a dream." The classification is used with reference to dreams in which the predominant character differs from some of the parts.

[2] The following are examples of dreams classified under these heads:

Personal. (C.) March 28. "Mrs. S. lunches with us and we have coffee cups which do not match."

Professional. (S.) March 30-31. "Dreams of demonstrating the Wheatstone stereoscope to Prof. and Mrs. X. Neither person well marked in my dream."

Trivial. (S.) March 29-30. "Dreamed of a large party (large as a S. S. picnic), being out boating on L. pond, or some place like it. The D.'s and others seemed to be there.

reavement one seldom dreams of the dead. The fact is often denied, chiefly because of the overwhelming impression produced by the infrequent dreams of this nature, yet it is proved by the experience of all whom I have questioned, by the examination of the dream records, and by the careful inquiries of M. Ives Délage,[1] the only writer, so far as I know, who has faced this problem. Monsieur Délage was profoundly impressed by the fact that during a long time of deep, personal sorrow, he never once dreamed of the friend whom he had lost, though he tried to provoke such dreams. There were at least two occasions during the months of investigation when, after days of very vivid memories, one of the observers confidently expected to dream of a friend who had died, yet actually dreamed only of the most trivial events. Both S. and C. during the whole time dreamed of the dead very seldom and in unimportant connection with other persons. The scrutiny of these dream-records, in fact, bears out M. Délage's conclusion: "*En régle generale, les idées qui ont obsédé l'esprit pendant la veille ne reviennent pas en rêve; on ne rêve des événements importants, que lorsque l'époque où ils préoccupait l'esprit à un haut degré s'est éloignée.*"

M. Délage's examination of the facts is very satisfactory, but his theory cannot be accepted. Every image or thought, he says, has a definite amount of energy. If this force be dissipated by directing to the image or idea the waking attention and interest, then none is left by which it may be lifted above the threshold of the dream consciousness; but if, as in the case of a trivial event, the attention has been diverted from it and never rested on it exclusively, then when most presentations are barred out by sleep, when the ordinary activities of thought, judgment and attention are wholly wanting, or at least lethargic, then the trivial image or thought will rise into consciousness, and be transmitted through the dream alchemy into the rich and varied material of the dream.

Passing by the untenable Herbartian assumption that "ideas" possess an independent existence of their own, one notices at once that this theory fails to account for all the facts. It is not true that one never dreams of subjects of vital interest to oneself. The home, the family, the school, profession or business figure in all our dreams, and yet are of deepest interest to ourselves. All these subjects, however, are connected with persons, places or things of which we have frequent sense impressions. In the "professional" dreams (which form so large a class of Table XX.)

[1] Essai sur la théorie du rêve. *Revue Scientifique*, 11 Juillet, 1891.

the laboratory, the apparatus, the books, the words are the "professional" elements of the dreams and all involve sense perception. On the other hand, the persons of the dead and of the long absent, as well as the intimate thoughts, hopes and ideals of the inner life, involve the waking imagination, thought, emotion and will, but not the sense perception. The connection of the dream with the waking life, so far as it can be traced, is therefore simply this: the dream will reproduce, in general, the persons, places and events of recent sense perception or of very vivid imagination—not the objects of ordinary imagination, of thought, of emotion or of will, so far as these are not also perceived objects. Furthermore, thoughts, emotions, experiences and personal relations that mean most to us are generally extremely complex, and depend for their reproduction on the integrity of very many lines of association. When a number of these are put temporarily out of function by sleep, it is next to impossible to bring about these complicated mental states, though less complicated ones can be reproduced with tolerable completeness.

III.

Up to this point the results of this classification of actual dreams have emphasized throughout the intimate connection of the dream with the waking life, and the essentially normal character of dream-activity. Yet the characteristics which distinguish the dream from the rest of the psychic life are no less prominent. Fundamentally these are three: the comparative feebleness of the attention and the will, the want of discrimination, and the relative lack of perception. Herakleitos says that each man has his own world in sleep, but a world in common with others when awake, and Fichte describes the objective world as created by the Absolute Ego, while the dream world is the creation of the individual I. Both sayings point to the essential feature in which dreams diverge from the rest of the psychical experience, the appearance of objective reality which prevents the recognition of their subjective character. Of this phenomenon, these three explanations have been suggested: most of the sense-impressions are lacking which furnish to the waking consciousness the common test of reality; the weakness of the thought activity facilitates that wrong application of the category of causality, implicit in the false reference of a phenomenon of imagination to an external cause and in the incorrect explanation of a sense experience; the diminution of attention prevents a scrutiny of images and ideas sufficient to the discovery of their subjectivity.

There are three stages in the dream illusion. The first is that simple objectivation which we have already described, and which is characteristic of all dreams. In the next higher stage, one attributes one's own thoughts and feelings to other individuals, sometimes explicitly recognizing one's own ignorance. Of this nature is the dream which Johnson naïvely describes; he was conversing with a man who said more witty things than he. Another time-honored example is that of Van Goens, who dreamed of himself in a recitation, in which after a most painful effort, he failed, only to hear his neighbor correctly answering the same question. Maury dreamed[1] of a person who told him the location of the place Mussidan, and of another who corrected his English sentence, "I called for you," by suggesting the idiomatic "I called on you." Both these bits of information Maury, in his waking moments, did not consciously possess.

These dreams in which the dreamer figures as the ignorant hearer of what he himself does not appear to know, are the most striking examples of this dream dramatization, but every conversation dream illustrates the same illusion.[2] Besides the general occasions of all objectivation, one observes here a failure of definite recollection. A vivid speech image of one's own thought is followed by the vivid image of some person, and the two are associated through lack of definite memory of the connection of the thought with oneself.[3]

Finally, the loss of the personal identity may become explicit. One imagines oneself to be another, or one is the double of oneself, and then there is a second self whom one sees or hears. The special feature of these dreams is sometimes characterized as a loss of self-consciousness. Spitta considers that the important differentiation of dream from waking is "*die während des Schlafens eintretende Aufhebung des Selbstbewusstseins*,"[4] but he makes the Herbartian distinction between *Bewusstsein* and *Selbstbewusstsein*, and declares that "*im Traume ist nur das Bewusstsein vorhanden, das Selbstbewusstsein nicht.*"[5] The fact is that explicit self-consciousness is almost always present in dreams, and that the individual ego is exceptionally prominent in the dream life. The loss of identity in dreams is not a loss, but a change or doubling of self-consciousness. It is probably correct to explain it by failure to recall the events of one's past history, or to localize them as referring to oneself. The

[1] *Op. cit.*, p. 115, seq.
[2] Cf. dream 193, quoted, p. 328.
[3] Cf. Maury, p. 116, *op. cit.*; "phenomène de memoire."
[4] *Op. cit.*, p. 262.
[5] Pp. 138-139.

result is a loss of remembered experience which suggests changed identity. Yet all the time one is conscious *that it is oneself* who has changed, or whose identity is doubled. Images of qualities, actions or words belonging to others are simply associated with the image of oneself; or the connection with one's own life, one's name, relatives, appearance is forgotten simply; or these images are projected outward, as when one goes to one's own funeral or listens to one's own conversation—but always there is an observing self.

An example of the first sort of dream, which is often a sort of romance dream, is the following:—

Dream 43. Sunday, March 22, 6 A. M. (Observer C.) (I am at first the reader of a story; then the servant and then the sister of the hero; finally, I assume my own character.) A hero goes through great distresses * * * runs up and down long stair-cases * * * has false information given him (here, the scene is partly in Italy, looking off to the Mediterranean); finally, alter eluding pursuers, the hero plunges through a sort of tunnel in the snow, which, a guide-post warns us, is very dangerous. I, his servant, follow; we are unharmed; we realize that the guide-post was treacherous. We reach, up many stairs, the home of the hero's beloved. He and I, now his sister, enter her room; she is not there. Then we enter the dining-room. He demands her. The family shudder, and one says, "*Du hast sie.*" I realize that she has died, and cry out to him, "*Ich felicitiere dich* * * * She is yours now. Here, *eine untreue Familie* has kept her from you."[1] "It now appears that all the hero's ribs are broken. I advise him to die and join his beloved, but for some reason he refuses.—Suddenly, the hero turns into my father, whose ribs are broken, and who is questioning whether he would not better die, while I (in my own character) refuse to let him."

A frequent feature of the change in one's own identity, and of the swift transformation of one dream figure into another, is the psychical experience known as paramnesia, which is very common in dreams, though what seems paramnesia (in the waking life and in dreams) may be often the actual memory of an incident of some forgotten dream. The paramnesia of dreams appears in various forms. People or things, absolutely unrecognized by the waking self, appear in our dreams as familiar; or a familiar person is endowed with qualities—personal or intellectual—which are quite foreign to him, and well-known localities are altered in certain features. Such

[1] I think that all this speech was in German, but only the quoted words were exactly remembered.

illusions, it is evident, may be traced directly to the weakness of attention and judgment; heterogeneous images occurring in close succession are referred to the same external object. Our recorded dreams are full of instances of this sort. There are instances of it in the long dream (No. 193) quoted on page 328.

A very detailed example is recorded in the following extract from Dream 78.[1] March 29-30. Immediately before waking. (Observer S.) "- - - - - -. When I left the chapel, I took a car. There was one track that turned off very soon to the right, the other swerved to the left and kept on up the hill. It seemed to me at first that I ought to be in a car that turned to the right, and had taken a wrong one. Then I remembered 'this is the car I have been in before, and I am all right.' As I looked back out of the car, I saw something like a galvanized iron ash-barrel rise rapidly at the end of a long iron arm, and enter the opening of an enormous tin tube - - like a reflecting telescope, thirty feet in diameter. This projected from the roof of a shed, or from between the houses. It seemed rusty and in one place somewhat broken. I remembered having been over the same line before, and to have seen the tube before. [Note.—Certainly paramnesia; never saw anything of the kind] - - - - -."

A favorite type of paramnesia, especially with one of the experimenters, was the dream of writing the records of a previous dream.

The incoherence of dreams proceeds from two main causes, one of which we have already discussed, the swift change of dream images through a process of modulated association. In this respect, the dream resembles revery, in which also the will and the judgment are in abeyance, and the subject half unconsciously makes *Ideensprunge* from one slender perch of thought to another. The second cause of incoherent dreams is effective also in revery; this is the intrusion of presentations into the chain of images and thoughts, and is a common feature also of dreams and of madness. As in the case of dreams, such presentations sometimes abruptly change the course of consciousness and sometimes are woven into its strands. So Maury tells the story of the madman on shipboard whose demon swore in Italian and made use of the real oaths of the sailors. But we may over-estimate the incoherence of dreams in our memory of them. It is easier to remember events which are in the line of our daily life than to recall the more or less fantastic or unusual train of dream-consciousness. So the gaps in some of our dreams may be breaks in memory.

[1] A very long and "modulated" dream.

The fantastic or absurd dream to which we have just alluded is not the same as the incoherent dream, for though all incoherent dreams are in a sense absurd, these are highly fantastic and improbable dreams, which move on in stately and uninterrupted measure. The following dream is one of this kind.[1] (I quote only part of it):

Dream 157, April 22-23, 6.05 A. M. (Observer S.) "I dream of a kind of intelligent elephant that is to be executed by electricity, - - - - and which I have charge of executing. - - - - The elephant is to be fastened to the Mach (rotation) apparatus, or rather a polished edition of it in the library, or a front room otherwise nearly empty. I get things ready so far as I can, and propose to go to work the next day. - - - - The elephant objects, and by degrees I come to talk the matter over with him. Am a little surprised, at first,[2] at his talking, but remember how intelligent an animal the elephant is reported to be, and find it very convenient to talk with him (he seems to appreciate the awkwardness of my position and talks rather kindly) - - - - not maintaining that he won't smash things with all his might when he feels the current, but as yet self-controlled. - - - - "

The following table gives the number of absurd dreams counted by both experimenters among those on record. Under the head "impossible" are included only natural impossibilities, like the speaking elephant, and the skull transferred by a surgical operation to the head of a living subject, not such dreams as those of meeting with people who have died. Of "very improbable" dreams an example is the "romance-dream."[3]

TABLE XXI.

	Rather Improbable.	Very Improbable.	Impossible.	Total Absurd.
S.	29	28	5	62
C.	42	35	3	80

The absurdity of dreams is clearly due to the weakness of the judgment. When our waking imagination confronts us with impossible images, though they have the vividness of the hypnogogic illusion, we yet correctly judge that they have

[1] Cf. also dream 28 (quoted page 325).
[2] Cf. discussion of surprise in dreams (p. 339).
[3] No. 43, p. 336.

not (at least in their apparent form) objective reality. In the dream we accept all images with a passive belief in their externality. It is untrue, however, to assert, as so many do,[1] that surprise is never felt in dreams. In the elephant dream just quoted, the judgment asserted itself in the surprise with which "at first" the talking of the elephant was heard, but the wonder was soon quieted by the vividness of the impression; it is interesting to observe that reflective thought was still present in the dream, since the peculiarity of the elephant was at once explained in true natural-history style. This possibility of dream thought, active enough to explain dream-images, but not keen enough to deny their externality, has appeared, already in the skull dream,[2] in which wonder at the *insouciance* with which the remarkable piece of surgery is performed, and at the deductions drawn from it, is accompanied by the complete acceptance of the fact and by much reasonable reflection on it.

It is true, however, that we constantly witness in our dreams, without a quaver of surprise, scenes and events of the most wildly improbable character. And just as the dreamer literally does not know enough to be surprised, so also his judgment plays him false in the recognition of the beautiful. I cannot recall a single distinct waking memory of any beautiful dream object, though it is common enough to dream of something as rarely beautiful which one recognizes through the waking recollection as merely fantastic or *bizarre*. It has happened to many of us to write dream verses which have seemed to the poetizing dreamer exquisitely beautiful, but which are recognized by the waking critic as sheer doggerel.[3]

IV.

One characteristic popularly attributed to dreams is open to question. It is by no means so certain as some of us think that the train of thought and imagery in dreams is swifter than that of the waking life. This assertion needs, of course, to be fortified, for there are innumerable instances which seem to prove the alleged swiftness of dreaming. The experience of Napoleon, who dreamed of a journey, a siege and a cannonad-

[1] Cf. Spitta, p. 138. His reasoning here is particularly specious. "Die Seele," he says, "kann nicht über Etwas in Verwirrung gerathen was sie selbst geschaffen hat." But this is exactly what "die Seele" experiences in dramatic and double identity dreams. (Cf. page 335 *seq.*)

[2] Dream 28, page 325.

[3] See papers by Miss F. P. Cobbe, Macmillan's Magazine, Nov., 1870, and April, 1871; and by Bessie A. Fecklen, Scribner's Magazine, May, 1891. The last refers to a book on "Dreams and Dream Stories," by Mrs. Anna Kingsford, which contains a dream poem of eighty verses.

ing, and woke while some slight explosion was still reverberating, to find himself in the midst of the journey; and of Mauchart, who dreamed the scenes of the reign of terror, his own arrest and trial and the falling of the guillotine, and was waked by the falling of some part of the bed upon his chest, —these are only two of the stories of which there is already a large collection. Maury tells these and others,[1] and gives two proofs from his own experience of the swiftness of dreams: he has often had long dreams while reading aloud to his mother, in naps which were so short that his mother never marked a pause. His brother, while talking in his sleep, used clipped words spoken so swiftly that they were barely understood.

When one tries, however, to render to oneself any definite account of a shortening of the perception of time, it is almost impossible to do so. Tissié's remark, "*Les impressions, etant plus vives, l'ideation se fait plus rapidement*," is not adequate, for it is only occasionally that the impressions are more vivid. It is therefore more than likely that only the memory-time, not the actual conscious process of the dream, is quickened. In the free association of the dream, the representation of one event immediately suggests that of some natural consequence, which objectively must be separated from it by a long expanse of time. But in the reproduction, after waking, of these chief links in the series of images (these *Hauptvorstellungen*), the memory adds the intervening links and one believes that one has dreamed them. The vivid images of the dream become representative of the others. "*Niemals*," says Spitta,[2] "*ist - - - - eine Traumvorstellung genau und bis ins Detail ausgebildet.*" The retrospective lengthening of the time is more natural because the number of images, as compared with thoughts, in a dream, is always large, and the presence of many images always makes the memory-time long.[3] It is possible also that in many dreams of this sort which are narrated, the early parts may have been dreamed before the sense excitation which is supposed to have occasioned the whole dream. So Napoleon may have been dreaming of a siege and Mauchart of a trial before the explosion or before the blow, which were simply incorporated into a dream already begun.[4]

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[1] Pp. 132–134.

[2] P. 248.

[3] Spitta, p. 169. Cf. pp. 158-171, for discussion of the subject.

[4] Probably some small portion of an experience, or even a single feature of it, stands as a sort of shorthand note, from which waking consciousness develops a more elaborate account. The bare words enumerating the stages of a long experience might

A scientific discussion of dreams may profitably omit many subjects which have especially interested most of the ancient and modern writers. The question whether "the soul ever sleeps" may be relegated to the realm of metaphysics, since there is little hope of observation minute or extended enough to answer it experimentally. The importance of the dream as a revealer of character is not a suggestive topic,[1] when one has discovered that both will and judgment are relaxed or altogether lacking, and that the very subjects of one's dreams are unconnected with one's deepest interests. The nature of prevision in dreams and the explanation of so-called "prophetic" dreams, have received a certain modern interest from the labors of Myers, Gurney and others.[2] The first remark to be made is that the number and the significance of these have inevitably been exaggerated. Such dreams are seldom accurately recorded, and the after experience (the so-called fulfillment) supplies details which one then believes that one has dreamed, even when the illusion does not rise to complete paramnesia.

In the second place it may safely be asserted that a large number of these dreams admits of a perfectly natural explanation. Previsions of illness were long ago assigned by Hippokrates to real organic sensations, with which are associated images of illness. A prevailing emotion persisting in sleep is associated with the image of some event as its occasion, and in the course of time this actually does occur. Perhaps some object perceived during the day and then forgotten, determines the character of this event. Délage[3] gives an admirable instance. A friend of his, a physician, had a terribly vivid dream of the death and burial of his father. He waked and found beneath the door of his room a telegram really announcing his father's illness. He hastened away, cared for his father during a severe illness and succeeded in saving his life. The whole has all the air of a prophetic dream until one discovers that the telegram had been slipped beneath his door during the evening preceding his dream; he had seen it without attending to it and the blue envelope[4] had suggested the dream, corresponding so closely with the reality.

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serve as such signs; just the word "Hamlet" even to the waking mind recalls some of the images of the drama. This tendency of consciousness, both sleeping and waking, to coerce experience into some reasonable shape is one of the most interesting aspects of the study of dreams.

[1] See, for opposite view, Sully, Cont. Rev., March, '93.

[2] Cf. Phantasms of the Living, Gurney, etc., II, pp. 380-443.

[3] *Revue Scientifique*, 11 Juillet, 1891; Essai sur la Théorie du Rêve, p. 42.

[4] This is a Parisian telegram.

Our dream records contain five cases of dreams in which there was a certain congruity between dreams and subsequent events. One is unimportant—a dream of the arrangement of articles in a magazine, and an unexpected note on the next day, with reference to arrangement of matter, at a time when this work was engaging considerable attention. In another dream, a remark is made about some one in whom the dreamer is much interested, so incongruous with the waking opinion that the dream is completely forgotten, and is not even recalled when, a month later, the same surprising story is heard and believed. This prevision, which was very exact, would have been entirely unnoticed but for the dream-record.

The other three dreams may be quoted. They all closely anticipate coming events, but are so essentially trivial and unimportant that there is no temptation to endow them with telepathic qualities :—

Dream 95. April 4–5, 4.10 A. M. (Observer S.) "Dream of visiting - - - - Wellesley College. M. and I are shown over the buildings. Among other things I am taken to see a little rotunda where a register of visitors is kept - - - - The floor of the rotunda is black and white marble, and has a few palms or other plants in it; it is rather poorly lighted. I think of Y. M. (a former pupil) as being there, and am not sure that I do not see her in the distance."

Weeks later, an actual visit to Wellesley was made, which had not been planned at the time of the dream; a rotunda, with the black and white marble floor and the palms, was seen; and a visitors' registry was found in another building. The suggestion of the dream was probably the casual mention the day before of an acquaintance at Wellesley, but there is no remembered occasion for the close likeness of the dream scenery to the actual surroundings. Yet it certainly is easiest to suppose some forgotten picture or description recalled in the dream.

Dream 161. April 24–25. (Observer S.) "Immediately before waking, I dream of being in O. - - - - I see Dr. Y. in a reclining chair on the sidewalk - - - - I speak to him and he refers to the next "Journal," saying that he can - - - easily get up matter on ether anæsthesia - - - - ."

The next day Dr. Y. (who lives forty miles away) makes a visit and actually speaks of the "Journal." The "prevision" is very close, but it is not easy to escape the conviction that it is a mere coincidence, for the "Journal" is a common object of thought and of perception, Dr. Y. is one of its contributors; and a piece of Dr. Y.'s apparatus may have been in the mind of the experimenter at about this time.

Dream 19. Friday, March 13–14, 11.35 P. M. (Observer C.) "I am coming from Cambridge on the cars with M. M., whose aunt will meet her at Harvard square. M. is coming from Newbury street, but will not go back home. She wears a red cloak."

Street-car dreams were not uncommon at this time, for the dreamer of them was going back and forth from Cambridge several times a week. But, on March 28, a fortnight later, Miss M. was actually met on a Cambridge car. The strangest part of this correspondence of dream and reality is that Miss M. had been more than a year in Brooklyn, and was at home for only two days. But even here forgotten suggestion seems a more likely explanation than telepathy. If in a large number of dream experiences, suggested by the affairs of daily life and peopled with the larger part of one's acquaintance, there should be no coincidences, that in itself would be a most remarkable fact.

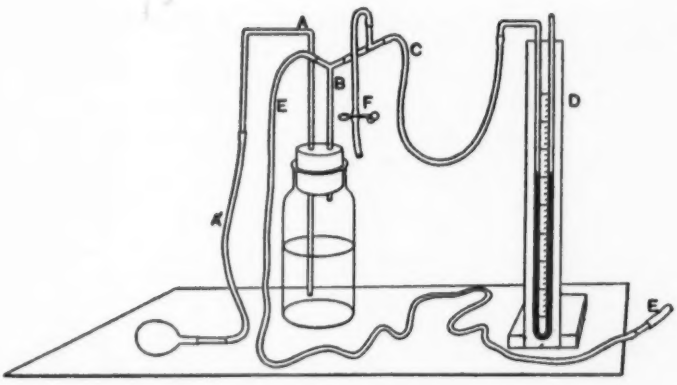
For these formulated results of the dream statistics of only two observers, no wide generality is claimed, and there are several dream experiences, of which the records show no trace, the dream, for example, that recurs always in the same form, and the dream of childhood in which the dreamer is himself a child again. Perhaps the most important effect of the whole study is the suggestion of questions which it does not answer and the indication of the useful possibilities of such study, as is in everybody's power, of the psychical phenomena of his own life. Such investigation must be slow and painstaking; it is not likely to lead to any startling results, but it cannot well fail to bring into stronger relief old truths previously accepted on hearsay, to correct traditional views, to supplement formulæ by facts, and at least to suggest some new aspect of psychological science.

ON THE PRESSURE SENSE OF THE DRUM OF THE EAR AND "FACIAL-VISION."

By F. B. Dresslar.

The purpose of this investigation was to determine in what way one is able to know when he is in the presence of objects even beyond his reach when the eyes are shut and blindfolded. This ability is especially strong with the blind, and sometimes spoken of by them as "facial vision." They not only receive much help thus in the way of protection, as is well known, but they are led to a more certain and definite knowledge of their environment than those who see are wont to think. A friend of the writer, blind from birth, could not only tell, for example, when he was approaching a forest and something of its general character, but he derived in some way much of the æsthetic feelings those who see get from sight alone. His horizon was by no means the indefinite blank homogeneity most often attributed to the blind, but it presented to him a variety approaching in a degree that of his more fortunate fellows. Much of this knowledge was due to association and inference, but much of it was gained through direct sense perception. Since the blind have no new sense, but impose on those remaining a greater task, it seemed important to determine in what degree of efficiency this capacity exists in those who see, and to analyze this sensation of "shut-in-ness" as far as possible. Prof. James says:[1] "The tympanic membrane is able to render sensible differences in pressure of the external atmosphere too slight to be felt as noise," and in a foot note says: "That the sensation in question is one of tactile rather than of acoustic sensibility would seem proved by the fact that a medical friend of the writer, both of whose *membranæ tympani* are quite normal, but one of whose ears is almost totally deaf, feels the presence and withdrawal of objects as well at one ear as at the other." In order to test this theory, a large number of experiments were made with a piece of apparatus, of which the following is a description:

[1] Psychology, Vol. II, p. 140.



Apparatus for Applying and Determining the Least Observable Amount of Atmospheric Pressure on the Tympanum of the Ear. A wide-mouthed bottle was corked with a rubber cork, which was pierced with two holes. In these holes two glass tubes were fitted, so that one (A) reached near the bottom of the bottle within, while the other (B) was much shorter. Tube (B) ended without in a double Y. Each of these openings and also the one at (A′) was fitted with a rubber tube. Tube (A′) ended in a bulb. (C) was connected with a manometer, which was fastened to an upright standard (D) and partially filled with ink. Behind this glass tube a milimeter scale was so adjusted that an observer could note the amount of the rise and fall of the liquid in the tube.

A long tube at (E) ended in a soft nozzle prepared to fit easily but closely into the external meatus of the ear. The one at (F) could be opened and closed at will to relieve any pressure caused by adjusting the tube (E) in the ear. In order, then, to determine the least perceptible amount of pressure, it was only necessary for the subject to adjust the tube in his ear, and, after the operator had restored the equilibrium, to indicate by a signal when he felt the pressure which was applied by the operator on the bulb. The height to which the column of ink was raised thus measured the pressure exerted. After each trial the subject removed the tube from his ear for a short time, then the process was repeated for ten trials.

The following table shows the averages and average variations of pressure, with F. B. D. as a subject, for a series of seven observations, each series consisting of ten trials for each

ear. These were taken irregularly for a period of a month or more:[1]

Right Ear.				Left Ear.			
No.	Height of Column in mm.	Av. Variation in mm.	Amount of Pressure in grams.	No.	Height of Column in mm.	Av. Variation in mm.	Amount of Pressure in grams.
1	33.	3.9	.32	1	50.4	1.66	.47
2	44.6	3.4	.43	2	57.2	2.	.54
3	32.8	1.84	.31	3	40.8	1.8	.38
4	38.6	1.48	.36	4	42.2	.94	.40
5	38.2	1.6	.35	5	41.	1.	.39
6	36.	2.4	.34	6	36.2	2.24	.34
7	41.8	1.44	.39	7	30.	.6	.29

Conclusions from the Above Experiments: 1. That it is a mistake to ascribe to the tympanum a very great delicacy in the perception of pressure. 2. That the sensation of being "shut in" is not due to tympanic pressure, because it is not possible for so much pressure to be exerted on the tympanum by merely approaching an object.

Suggestions: 1. The method used suggested a much better plan for determining the threshold for minimal pressure than heretofore used. It was noticed that the height to which the pressure could be raised before being felt depended on the suddenness and rapidity with which the pressure was applied. By using a regulated mechanical pressure, the variations in the threshold for pressure as affected by rate could be more clearly determined. 2. The bilateral asymmetry noticed in the above records suggests additional evidence in functional asymmetry of the two sides of the body.

Having seen that tympanic pressure could be wholly excluded as an element in the sensation, the next thing was to find a method by which the relative value of the three remaining possible elements, viz., temperature, sound and "facial vision," could be determined. This was accomplished with the following apparatus:

Apparatus and Method for Determining the Effect of Temperature, Sound and "Facial Vision." A light frame-work of wood was made four feet long and one foot wide, and divided into four spaces, each one foot square. The first one of these spaces was left open, the second latticed with strips

[1] It should be said that practically the same results as the above were obtained in the same way with two other subjects, but unfortunately the figures were lost.

three-quarters of an inch wide, and with spaces of one-half of an inch between them; the first space was closed solidly with a panel of wood, while the fourth was filled with a wire screen. The frame was then suspended from a high ceiling by four strings, fastened in pairs as guys (so that it would swing lengthwise easily and without swerving), and made to swing low enough to be opposite the face of the subject. A silk thread was fastened to the ends of the frame, and passed over small pulleys inserted in standards set some ten feet from each end of the frame. The two ends of the strings were then tied together to furnish the operator an easy and noiseless method for shifting the frame to bring the different spaces opposite the ear and face of the subject.

The method of work was as follows: The subject with closed eyes was blindfolded in such a way that little or none of the face but the eyes was covered; he was seated comfortably with his face at a distance of two or three inches from the path of the frame, and asked to judge between two spaces irregularly presented. He was required to indicate his judgment by a prearranged system of signs in order to prevent any reverberation of the voice which might vitiate the results. The first set of judgments were thus taken, and were for the purpose of finding the degree of power to distinguish between the three following pairs: Open—lattice; lattice—solid; solid—wire. The accompanying table shows the discriminative ability thus drawn out for different subjects:

Subject.	Open and Lattice.				Lattice and Solid.				Solid and Wire.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	65	15	59	25	58	2	56	0	45	0	46	2
O. C.	72	47	74	46	33	13	28	14	21	4	14	9
F. B. D.	53	24	58	17	69	1	70	4	73	0	77	2

Explanation of Table.—The figures in the columns marked *R* indicate the number of correct judgments, those in columns marked *W* incorrect. For example, when J. A. B. judged between the open and lattice spaces, he made 65 right and 15 wrong judgments in 80 presentations of the open, and 59 right and 25 wrong in 84 presentations of the lattice; while judging between lattice and solid, he made 58 right and 2 wrong judgments in 60 presentations of the lattice, and 56 right and no wrong judgments when the solid was presented, and so on for all the others.

After each set of experiments, the subjects were asked to state as nearly as possible just what they thought they had

used as a basis for discrimination. This was done for the double purpose of gaining therefrom hints as to method, and also as to the accuracy of introspection and the ability to analyze these sensations. The following answers cover the main points made:

J. A. B.—1. I could not hear the movements of the apparatus. 2. Reflected sounds formed the chief basis of judgment. Footsteps outside have different qualities of sound when the different parts are opposite the face. 3. There is a sort of a "shut-in" feeling when a closed space was presented, and a feeling of relief came when it changed to an open space. 4. Sometimes I seemed to see the frame through the side of the face, a sort of "facial vision." 5. There occasionally seemed to be a difference in temperature.

O. C.—1. I heard no sound from the moving apparatus. 2. There seemed to be a difference in temperature, and this feeling was located in the ear. 3. At times seemed to visualize. 4. Occasionally felt as if something were being pushed into my ear. 5. At times there was a feeling as if the head were being put in a box. A distinct "shut-in-ness."

F. B. D.—1. There was no sound caused by the apparatus as it moved past the face. 2. The basis for judgment seemed to be a difference in the quality of the reflected sounds. 3. When all without was very quiet, the feeling of "shut-in-ness" seemed present, and sometimes seemed localized in the temples. This localization was perhaps due to muscular tension, caused by straining the attention.[1] (This fact may in some measure account for the belief in "facial vision.") 4. Occasional differences in temperature seemed present. 5. The feeling of "shut-in-ness" arising from the nearness of the parts of the frame, is wholly different from that of direct pressure on the tympanum. 6. In later experiments, when my ears were closed with cotton, it became a difficult task to prevent my attention from wandering, because of the lack of any change in the sensation,—nothing to tie it to, as it were.

It now became necessary to devise a method of excluding the temperature and "facial vision." This was done in the following way: The ear was covered and fitted with a soft piece of cloth, in which a hole was made opposite the meatus. Then a piece of thick, soft cardboard, sufficiently large to cover the entire side of the face and neck, was cut so as to fit closely around the base of the ear to hold the cloth in place

[1] See James' Psychology, Vol. I, p. 434 f.

and shut the face from the frame. It was thought now that if the subject, after being blindfolded as before, could distinguish the separate spaces, that it would depend on differences in sound, and that the loss in discriminative ability would be due to shutting out temperature and "facial vision." The following table shows that the power of discrimination was very slightly, if at all, reduced:

Subject.	Solid and Lattice.				Solid and Wire.				Open and Lattice.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	23	0	26	0	20	0	18	0				
O. C.	23	4	21	8	24	8	22	9				
F. B. D.	92	5	90	2	52	0	51	1	21	6	23	5

It should be taken into consideration that the cloth and cardboard on the face, despite all care to prevent, caused some little distraction of the attention, and in this way would account for some loss of discriminative ability, but a comparison of this table with the foregoing one shows very little change.

In order to test the matter more thoroughly, the face was exposed as at first, but the ears were both[1] tightly stopped, and the accompanying table shows that the power to distinguish was lost entirely:

Subject.	Solid and Lattice.				Solid and Wire.				Open and Lattice.			
	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.
J. A. B.	34	34	35	33								
O. C.	53	47	43	65								
F. B. D.	82	84	67	62	54	45	52	49	29	31	34	28

It was thought to be waste of time to complete this table of experiments with J. A. B. and O. C., for the obvious reason

[1] The fact that it was necessary to stop *both* ears tightly with cotton before the ability to discriminate was lost, suggests that this may account for Prof. James' medical friend's ability previously referred to.

that the previous experiments and those here with F. B. D. were conclusive.

The table proves beyond a doubt that with the subjects named, the basis of judgment was due to differences in sound.

The question arose whether or not these were wholly objective sounds, or partly subjective. This point was not fully determined, but the following observations were made: An acoustic telephone was fixed with the receiver near the frame, and the subject with the other end at some distance away. There was then no distinguishable difference.

Also it was noticed throughout all the experiments that too much noise in the room or outside was unfavorable to correct judgments, but this was perhaps chiefly due to distracted attention as well as to the general resonance of the room.

ON REACTION-TIMES WHEN THE STIMULUS IS APPLIED TO THE REACTING HAND.[1]

In his classical study of reaction-times Exner was led to the opinion that the reaction-time for electrical stimulation is longer by about 10σ when the stimulus is received in the reacting hand than when it is received in the other hand.[2] This result, as Exner himself remarks, is just the opposite of what might be expected, and, as his conclusion rests upon relatively few trials, invites a repetition of the experiment. The purpose of this short paper is to give the results of a careful repetition.

Apparatus and Methods. The reaction-times were measured with the Hipp chronoscope, set up to start at the closing of the electrical circuit and to stop at the breaking. The regularity of the chronoscope was tested with the Cattell fall-apparatus, adjusted to give an interval of not far from 130σ, but as the question to be answered was one of relative and not absolute times, it seemed unnecessary to seek an absolute correction of the chronoscope. The conditions were the same in all cases for the experiments to be compared, and the constant error remaining cannot amount to more than 10σ. The stimulus was an induction shock of moderate intensity, applied by small platinum electrodes let into the surface of the knob of the reacting key, and of a similar key upon which the finger of the passive hand rested. The stimulus was applied alternately to the end of the finger in the reacting and in the passive hand, the alternation being used to equalize practice, fatigue, strength of shock and all other conditions as exactly as possible. A Pohl commutator in the secondary circuit made it easy to shift the stimulus from one hand to the other without disarrangement of the other parts of the apparatus. Tests were made upon three subjects, S., B. and R.

[1] The essential experiments shown in Table I. were made by Mr. J. F. Reigart. The statement of the results, however, has been left to me. E. C. S.

[2] Exner: Experimentelle Untersuchung der einfachsten psychischen Processe, Pflüger's Archiv, VII., 1873, pp. 622-623, and 655-656; also Hermann's Handbuch der Physiol.

All had worked before at reaction-time experiments, and none was ignorant of the object of the tests, but it is believed that this had little or no effect upon the result. In all cases the stimulus was preceded by a ready-signal, and the subject responded as quickly as he could. In taking the tests, Exner's own method of excluding irregular reactions was rigorously applied. The subjects, after each reaction, passed judgment upon it, calling it "good," "too late," or "too early," as the case might be. All the reactions accepted as good by the subject, and those only, were used in computing the averages.

Results. The results of the tests are given in the following tables:

TABLE I.

Stimulus alternately in the right and left hands. Subject selected the satisfactory reactions.

REACTION WITH RIGHT HAND.

	Stimulus in Right Hand.				Stimulus in Left Hand.			
Subject.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
S.	30	10	128.4	8.	25	10	142.7	9.
B.	27	22	145.8	10.1	25	21	143.4	6.8
R.	14	11	153.3	6.6	14	13	150.0	6.1

REMARK: Subject S. seemed more critical in selecting when the stimulus was in the reacting hand. The shock seemed to him stronger in the left hand.

REACTION WITH LEFT HAND.

	Stimulus in Right Hand.				Stimulus in Left Hand.			
Subject.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
S.	15	10	141.9	10.9	26	10	140.2	5.8
B.	24	22	148.2	10.9	24	24	131.3	8.1
R.[1]	15	11	166.7	13.9	15	15	163.0	29.3
R.[2]	25	19	138.1	10.9	26	23	136.6	8.6

REMARK: Same remark as above for subject S. R.'s first and second series were taken a little more than a month apart.

It will be observed that the results of this table are opposed to those of Exner, except in the cases of B. and R. when reacting with the right hand, and even in those cases the excess when the stimulus was in the reacting hand is too small to be regarded.[1] In the case of S. when reacting with the right hand, and of B. when reacting with the left, the differences are larger than those found by Exner and in the contrary direction. The remark on the reactions of S., especially with reference to his greater strictness of selection when the stimulus was in the reacting hand, would at first sight seem to weaken the force of his figures. This strictness was, however, certainly not due to conscious prejudice, and may very well point to a keener sense of the difference between prompt and delayed reactions when the stimulus and reaction are on the same side. If this is the case, the point would tend to support rather than oppose the statement which the rest of the table justifies, namely, that it makes very little difference which hand receives the stimulus. The variation of the experiment giving the second half of Table I. (not tried by Exner) excludes the possibility that previous practice in reacting with the right hand should have given that hand an advantage in quickness.

The same practical equality of the reaction-times when the stimulus is given alternately right and left was shown, though by somewhat uneven figures, in an experiment made upon

[1] In order to facilitate comparison, Exner's results have been put in the form of Table I.; the full figures will be found in the place cited above, pp. 655-656.

REACTION WITH RIGHT HAND.

	Stimulus in Right Hand.				Stimulus in Left Hand.			
Subject.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.	Total No. of Reactions	No. of reactions Selected.	Average.	Av. v.
E.	8	6	139.0	4.1	17	11	128.3	5.4
v. B.	12	4	146.9	3.7	12	3	138.1	4.3

The subject E. was Exner himself; the subject v. B. had never engaged in such experiments before. In the case of v. B. some of the reactions were called *ziemlich gut* or *nahezu gut*; these Exner excluded from the averages, as also two reactions that were called good by the subject, but were considerably larger than the rest.

another subject and at an earlier time by Dr. Sanford.[1] In this experiment a genuine touch with a hard rubber point was used as the stimulus instead of an electric shock. The result is given in Table II.

[1]The experiment here referred to was made with a number of others soon after the opening of the University, and before the full equipment of the laboratory. The times were measured with the Hipp, but the battery used was not of the most constant kind and the regulating apparatus was the antiquated Hipp ball-dropper. The results given in Table II., however, are regarded as valid for purposes of comparison between right and left, for the stimuli were alternately applied and both quantities may be assumed to have been equally affected by what changes occurred. At the same period experiments were tried with electrical stimulation of the finger on each side, but it was difficult to secure uniform intensity of shock and for that reason less confidence is felt in them. They were made upon three subjects, with some variety of conditions. With two of these the differences were small and now in one direction, now in the other. With the third—the least experienced and probably, also, the least informed as to the purpose of the experiment—part of the figures agree in direction with those of Exner, as appears in the following little table. The figures given are those for averages, corrected as in Table II., but in no case does the correction alter the direction of the difference.

In the first, third and fifth series the results for this subject agree in nature with those of Exner.

	Stimulus Right.		Stimulus Left.	
	Average.	Av. v.	Average.	Av. v.
Reaction with right hand, pressing key,	155.3	11.7	146.6	9.9
	147.9	12.3	147.0	10.5
	144.7	20.1	130.0	10.8
Reaction with left hand, pressing key,	173.1	13.5	166.0	17.3
Reaction with right hand, withdrawing finger from key,	183.4	15.1	178.8	11.4

TABLE II.

Stimulation by contact, alternately right and left. Stimulus applied in the first set to the back of the hand; in the second set to the second joint of the thumb. Reaction in the first set by pressing a key with finger of right hand; in the second set by removing finger from the key. Subject A.

	Stimulus in Right Hand.			Stimulus in Left Hand.		
Subject.	No. of Tests.	Average.	Av. v.	No. of Tests.	Average.	Av. v.
A[1]	25	118.4	21.7	25	132.4	19.0
	22	**113.9**	**13.8**	**22**	**127.7**	**13.7**
A[2]	24	141.9	19.4	24	139.7	19.9
	21	**135.7**	**13.7**	**21**	**136.4**	**16.0**

The corrected results in this table, indicated by heavy faced type, were reached by rejecting from the corrected averages the three reaction-times most variant from the original uncorrected average.

Of the same tenor is the result of an experiment upon R., when no selection by the subject was attempted. The stimulus was applied alternately right and left; the reaction was with the right hand. When the stimulus was in the reacting hand, 22 tests gave a mean of 124.4σ with an average variation of 8σ; when in the other hand, a mean of 140σ with an average variation of 7σ. Casting out from each 22 the three reactions most variant from the mean, the remaining 19 average respectively 124σ and 140σ, and the average variations are 6σ and 5σ.

With this agree also some measurements made by Dumreicher,[1] though his point was the discovery of the conditions giving the greatest uniformity of results rather than the greatest quickness. He found for reaction by withdrawing the finger from the key (the stimulus was given through electrodes in the knob of the key) 126σ when the stimulus was in the reacting hand and 158σ when in the other hand; and for reaction by pressing the key, 173σ and 187σ respectively. His measurements were made with Ewald's chronoscope.

[1] Zur Messung der Reactionszeit, Inaug. Dis., Strasbourg; pp. 43, 44.

EXPERIMENTS UPON PHYSIOLOGICAL MEMORY BY MEANS OF THE INTERFERENCE OF ASSOCIATIONS.

By John A. Bergström.

The interference of associations is a fundamental fact of the nervous system and so of frequent occurrence. In learning a new way of playing an instrument, or a second system of shorthand, or a new style of writing, or in substituting a new movement in gymnastics, or any game of skill; in general, in changing from an accustomed way of doing a thing to a new way, the old habit resists displacement; and if we act hastily, or are fatigued or absorbed in something else, we are liable to act in the old way. It is not evident, however, without experiment that the old habit interferes with the formation of the new one. It is easier to act in the old way, but it is not evident that we cannot learn a new way, if we try, as easily as we learned the old one. The reason we follow the old way may be a disinclination to effort. In the small field of the present experiments it will appear, however, that in spite of every effort a very decided interference takes place when we attempt to associate a new reaction with an old stimulus, and that the interference is so constant that it may be made a means of measuring memory just as the greater ease in relearning something once learned has been used in other experiments. The confusion which results in the minds of young students in presenting a subject to them in more than one way is an illustration in another field. Much that has been attributed to the decreasing of memory power with age, as in the case of learning languages, should, perhaps, come under the category of interference. Interference, as well as practice, should be taken into account in all psychological experiments when the same stimuli or signs are used in different relations.

Methods and First Experiments. The method to be described was developed in studying the errors of an experiment upon quite another problem. In this, use was made of a test based upon the method of finding distinction and choice

times by means of an ordinary pack of playing cards.[1] Unprinted cards with the best slipping qualities were procured and made up into packs of 80 each. There were ten kinds of cards in each pack, each kind marked by an abstract word printed at the top. The cards were well shuffled and sorted as rapidly as possible into ten piles, each pile having all the cards with a given word. Chance determined the order of the piles, and this was in the main different in successive experiments. Each experiment consisted of sorting in quick succession two packs bearing the same words into different piles, which we may represent by letters as follows:

1.					2.				
A	*B*	*C*	*D*	*E*	*G*	*J*	*H*	*A*	*F*
F	*G*	*H*	*I*	*J*	*I*	*C*	*E*	*B*	*D*

In sorting the second pack the time was longer and the subject appeared confused, and had a tendency to place the cards in the positions they had had in the first half of the experiment. Two different reactions were associated with the same sensory stimulus and interference took place. The experiment was not directed to the study of this phenomenon, and the indirect results we obtained simply established the fact of interference. During the last of March and the first of April, '92, eight to ten experiments a day were made at regular intervals for from three to eight days in the case of the following persons. The first column gives the average time in seconds for sorting the first pack, and the second the excess of time for the second pack:

	1.	2.
W. L. B.,	158.7	17.5
L. L. B.,	122.5	10.1
A. F.,	127.8	18.
E. N. B.,	158.	16.8
W. O. K.,	145.9	3.5

When the second pack bore different words from the first there was practically no interference, as appears in the results for another subject, F. B. D.

The average of nine experiments on nine different days upon F. B. D. gave a difference of 30.25 seconds when the first and second packs were alike; and for three days in which two packs with different words were sorted successively, a difference of only 0.53 seconds. The pack with different words was procured to avoid the interference due to mak-

[1] Science, VIII., 1886, 238.

ing the first associations. If the words were different in the two packs and the retardation took place nevertheless, a part of it might be supposed to be due to fatigue. If no lengthening of time occurred, then the differences in the experiments with the packs having the same words would all be due to the interference of the associations. The decision of this point, however, must be left to the direct experiments presently to be described, since the evidence from these early indirect ones is incomplete. In 23 and 30 experiments respectively with A. F. and W. O. K., a second pack, different from the first, was sorted more quickly by 2.7 and 3.1 seconds. A part of this difference may be due to a difference in the packs, since, unfortunately, the order in which they were taken was not changed to make compensation.

Later Experiments. In order that the interference of associations might be under favorable experimental conditions, the sensory and mechanical parts of the process were simplified as much as possible. Instead of words, pictures of common objects were drawn at the top of the cards, and care was taken to have every picture different from every other. One set, for example, consisted of a house, a book, a coffee-pot, an acorn, a palette, a clothes brush, a glass, a safety-pin, a fan, and an oak leaf. They must be easily distinguished and not liable to grouping or other mnemonical devices; and all the pictures in all the sets must be different. The cards were held face up, and thrown upon a table covered with a rough white cloth to keep them from sliding.

The first problem studied was the rate of decrease of the interference with increasing intervals of time between the first and second sortings. An experiment consisted of two parts: the first, the sorting of a pack of cards into piles in given positions; the second, the sorting, after a certain interval of time counted from the end of the first to the beginning of the second parts, of a pack having the same pictures into piles in entirely different positions. Seven, and in one case eight, different intervals were used ranging from three to 960 seconds. One experiment a day for each interval was taken at a given hour. This required eight double sets of cards, the actual number of which was reduced one-half by having pictures at both ends of the cards. A complete record of the positions occupied by the cards from day to day was kept so that no repetitions should occur till after several days. Since a card can occupy any one of the nine other positions than the one in which it first occurs, there are ten arrangements possible without a single repetition. It would probably make the results more regular to have a sufficient number of sets to make any repetition whatever unnecessary. The relative

results sought in this case should, however, be nearly the same, since all the packs received approximately the same treatment, though we may expect to find the probable errors made somewhat larger thereby. Two minutes' rest was allowed between the experiments on different intervals, and all together they required nearly an hour a day. To allow for practice, fatigue and changing conditions in the hours, the experiments were begun with the different intervals in turn. The cards were usually picked up from the table irregularly, but sometimes shuffled, and in either case care was taken that no two cards with the same pictures should come together and that there should be no recurrence of orders in the packs. The results obtained appear in the following tables:

Table I. Subject, F. D.; member of the Pedagogical Department; unpracticed in this kind of experimental work; not aware of the purpose of the experiment till the end; refrained from any speculations about the matter. In sorting the first pack, the ability to throw the cards without looking at the piles was reached in some cases; usually all but two or three positions were known. There was no tendency to employ mnemonical schemes. In the second sorting, false movements were very numerous at first; some tendency to employ mnemonics; so strong a habit of looking towards the former positions that cards directly in front were often hard to recognize. The records for the first four days were only partial and very irregular, and so were not used in making out the average. The rest number 16 days, or 112 experiments. The first horizontal row of figures in all the tables gives the intervals between the first and second packs. The second and third horizontal rows of figures, with the exception of the first and last figures, give the corresponding difference between the sorting of the first and second packs, each figure being the average of eight days' records. Next comes the average of the differences, then the probable error of these averages. If we assume that at each interval the same quantity is measured, and that the variations are due to accidental errors, Chauvenet's criterion for the rejection of observations may be applied. The result is given in the last row. The first figures at the left (Av. P.') give the average of all the first packs for the time indicated, and so show the result of practice. The figures at the right (Av. D.) give the average of the differences, which, in this case, show a slight increase rather than decrease with practice. The condition of making the greatest possible haste was not strictly complied with, since the subject had a tendency to select the rate he thought would enable him to make the best time. All figures represent seconds. The averages of the differences from Table

I. are shown in graphic form in Fig. 1 of the diagram at the end of the paper.

TABLE I.

	(Av. P.')	3''	15''	30''	60''	120''	240''	480''	Av. D.
Oct. 25-Nov. 2.	80.52	26.2	23.3	17.93	17.16	20.03	14.4	13.06	18.87
Nov. 3-12.	72.62	27.03	19.88	23.45	25.	20.18	16.35	14.06	20.67
Average of Dif.		26.62	21.59	20.69	21.08	20.11	15.88	13.56	
P. E. of Av.		1.23	1.25	1.50	1.45	1.18	.96	.97	
Corrected by Chauv. Crit.		26.64	22.64	22.02	21.8	19.10	16.25	15.29	

Table II. Subject, O. C.; member of the Pedagogical Department; unpracticed in this kind of work, and also unacquainted with the purpose of the experiments. As in the first case the first four days are omitted in making up the

TABLE II.

	(Av. P.')	3''	15''	30''	60''	120''	240''	480''	Av. D.
Oct. 24-Nov. 2.	67.59	18.46	15.33	15.83	14.39	13.31	13.72	14.28	15.05
Nov. 3-12.	63.72	16.2	11.63	12.54	12.31	8.00	9.72	8.42	11.29
Average of Dif.		17.33	13.48	14.18	13.45	10.66	11.72	11.34	
P. E. of Av.		.91	.74	1.17	1.26	1.26	1.25	1.18	
Corrected Results.		17.39	13.48	12.42	12.2	9.14	10.2	9.27	

table. Eighteen days' records remain — 126 experiments. The averages in the second and third horizontal rows are made on the basis of nine records for each. Here also the tendency to regulate the speed so as to make the best time

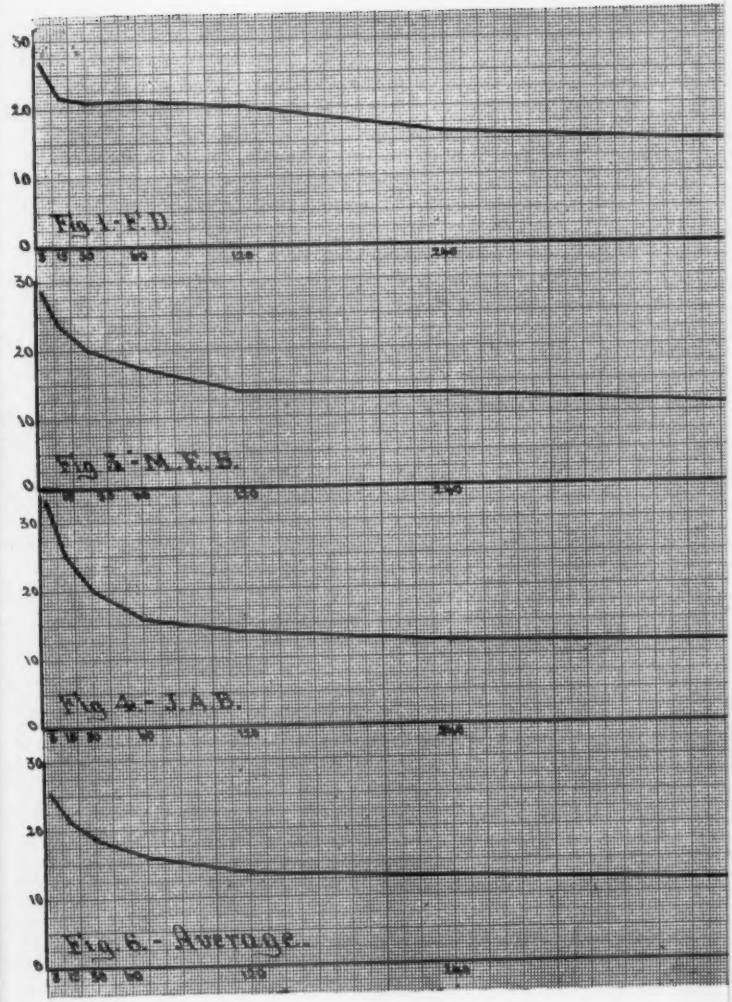

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Fig 1 - F. D.
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Fig 3 - M. E. B.
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Fig 4 - J. A. B.
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Fig 6. - Average.

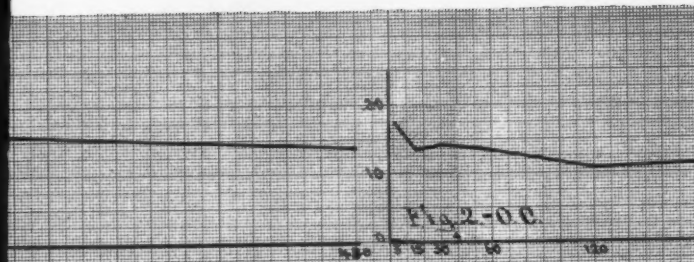


Fig. 2.-O.C.

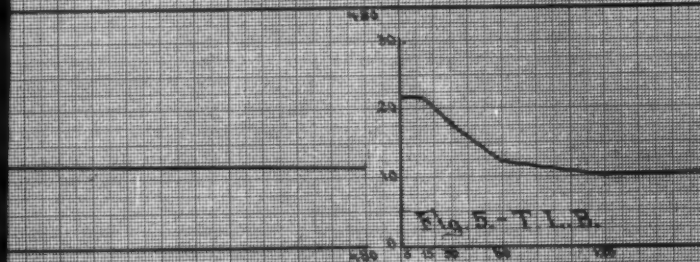
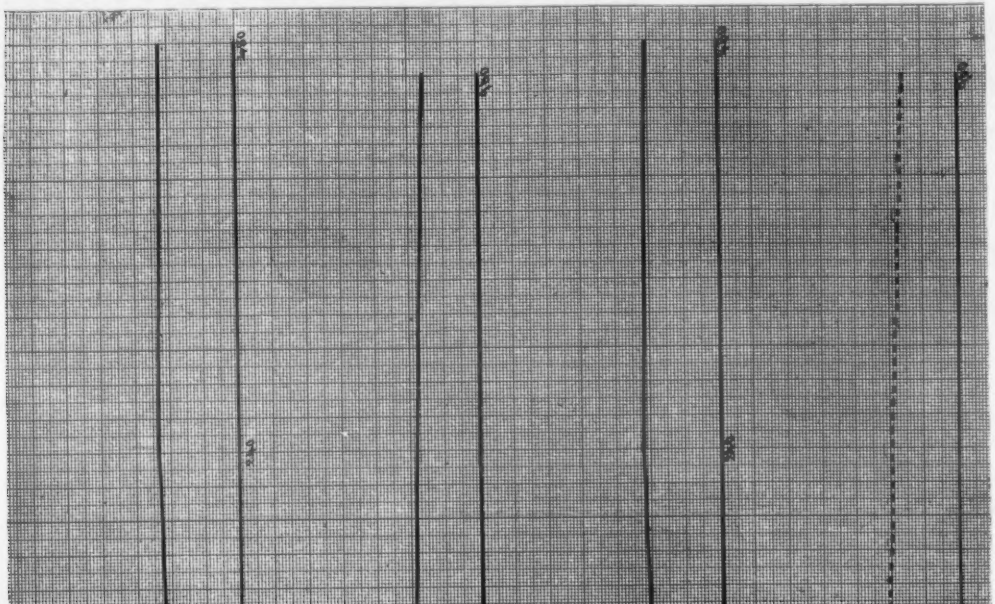


Fig. 5.-T.L.B.



was noticed. The eyes were very active, following the throwing of each card, as far as the experimenter could judge. The nice arrangement of the piles would show the same. The differences are comparatively small, and as is seen in column (Av. D.), there is a considerable decrease in the average of the differences. The constant use of the eyes did not permit so strong a habit to be formed; and their quick motions probably indicated an individual peculiarity of temperament and muscular control, though these may be dependent upon the absence of strong tendencies in the directions of the previous positions, which seems to the writer most probable. The averages of the differences from Table II. are shown in Fig. 2 of the diagram.

Table III. Subject, M. E. B.; acquainted with this kind of work from acting as experimenter in the experiments of Table IV.; also trained in sorting other cards than those used. All the records taken are made use of in the table, since there was no need of allowing for practice in the manipulation of the cards. By watching the eyes of the subject, the time when all the positions were learned could be noted down. The average of this time for the last three days is 34 seconds for the first pack; the last was rarely learned entirely, even with the longer intervals, and then only after 65 or 70 seconds. The greatest possible speed was attempted, and there was no conscious adjustment of the speed to make in the judgment of the subject the quickest time. There are 12 days' experiments in all—94 records. The 960″ interval has only 10 records. The averages in the second and third rows are based on six records each, except as just stated, those of 960″ interval. The averages of the differences from Table III. give Fig. 3 of the diagram.

TABLE III.

	(Av. P.)	3″	15″	30″	60″	120″	240″	480″	960″	Av. D.
Nov. 8-13.	60.08	29.2	23.66	17.23	19.03	13.71	12.83	11.2	10.22	17.11
Nov. 14-20.	59.37	28.38	23.65	22.55	15.66	13.43	13.36	8.96	5.36	16.42
Average of Dif.		28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79	
P. E. of Av.		.70	.92	1.20	1.01	1.31	1.23	1.62	1.11	
Corrected Results.		28.79	23.66	21.16	18.42	12.26	13.09	10.08	7.79	

Table IV. Subject, the writer. During all but the first seven days of the experiment the subject was engaged two hours a day with experiments upon others. Every effort at speed was made, and there was no conscious allowance for what might be the best rate. The records were taken between 7 and 8 A. M. No records are omitted to allow for practice; but three days' records are omitted in the latter part of the time, because of some irregularities in health and sleep, which made the circumstances abnormal. The average, with these included, is given below for comparison. The table is based on 21 days' records—147 experiments. The averages of the differences from Table IV. give Fig. 4 of the diagram.

TABLE IV.

	(Av. P.')	3''	15''	30''	60''	120''	240''	480''	Av. D.
Oct. 13-20.	84.15	31.07	26.71	16.69	12.19	14.28	11.29	8.71	17.37
Oct. 21-31.	65.68	35.86	27.07	25.60	19.57	11.54	12.29	13.39	20.76
Nov. 1-9.	61.98	31.71	21.76	18.97	15.53	15.43	12.56	11.03	18.14
Average of Dif.		33.09	25.18	20.42	15.76	13.75	12.04	11.04	
P. E. of Av.		1.20	1.07	1.10	.83	.81	.65	1.04	
Corrected Results.		32.15	25.18	19.50	15.76	13.75	12.00	11.04	
Ave. of all 24 Records.		32.65	25.75	20.08	16.3	14.94	12.08	11.54	

Table V. These records, though taken first, are placed last, because the arrangement of the experiment is different. Two experiments were taken every two hours, between 8 A. M. and 6 P. M. The cards with words instead of pictures were used. The experiments were so distributed that two records at each time of the day were to be taken for each interval. It was hoped by this means to find not only the rate of disappearance of neural memory in general, but also the difference for different times of day. The plan of the experiment is open to objections on other grounds, but the fact that the same cards were used incessantly, and that only two hours

were allowed for the habit to decrease in intensity before another trial was made, would lead us to expect great irregularities. The subject was T. L. B.; familiar with psychological experiments, and in good practice with the cards; acquainted in a general way with the purpose of the work. To Table V. corresponds Fig. 5 of the diagram.

TABLE V.

	(Av. P.')	3″	15″	30″	60″	120″	240″	480″
June 1-13.	80.86	21.85	21.42	17.88	12.25	10.02	11.2	10.25
P. E. of Av.		2.28	2.19	1.87	2.14	2.02	3.96	1.44

There are 66 experiments in all; 10 or 11 experiments each for all, except the last two intervals, for which there are only 7 and 4 respectively.

These differences might, perhaps, be supposed to be due in large measure to fatigue. To test the effect of fatigue or practice, some experiments were made on successive days, as before, in which two packs with different pictures were sorted successively with the shortest interval, where fatigue would show itself if anywhere. Half the experiments began with one, half with the other pack, to compensate for any differences in them. The averages here as in the other experiments are in seconds. P^1 and P^2 are the first and second packs respectively. The first two averages include 12, the last two 16 experiments each.

TABLE VI.

	P^1	P^2	$P^1 - P^2$
O. C.	63.53	62.94	.59
F. D.	66.8	65.37	1.43
M. E. B.	56.62	56.49	.13
J. A. B.	64.61.	63.14	1.47
Average.	62.89	61.99	.90

Thus when there is no chance for interference to take place the tendency is in all cases to sort the second pack a little quicker than the first. We may conclude, therefore, that the differences are not due to fatigue.

The attitude of the subject is here the reverse of that in the work of previous experimenters. In their experiments, his interest was to remember, in these to forget. The influence of the knowledge that something is to be remembered upon its retention is well known. The relatively large amount of interference throughout the series, and the slight diminution of the average of the differences, as seen in the right-hand column (Av. D.), makes it probable that forgetting—that is, the disappearance of habit—is not an active process of disintegration, but merely a letting alone. The rapid rate of disappearance at first was one of the most striking facts noticed in the preliminary experiments.

The uncorrected average differences, and their average for all the subjects, are given together for the sake of comparison in Table VII.

TABLE VII.

	3″	15″	30″	60″	120″	240″	480″	960″
F. D.	26.62	21.59	20.69	21.08	20.11	15.88	13.56	
O. C.	17.33	13.48	14.18	13.45	10.66	11.72	11.34	
M. E. B.	28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79
J. A. B.	33.09	25.18	20.42	15.76	13.75	12.	11.04	
T. L. B.	21.85	21.42	17.88	12.25	10.02	11.2	10.25	
Average.	25.54	21.07	18.61	15.98	13.62	12.78	11.25	[7.79]

Reference to the results corrected by Chauvenet's criterion shows that those of F. D. and O. C. follow the average in general more nearly than the uncorrected results; but the assumption on which the corrections rest is at best only approximately true. The average difference between the 3-second and the 8-minute differences is 14.28 seconds; and between the 1-minute and the 8-minute differences 4.72, which

shows that about $\frac{2}{3}$ of the decrease took place in the first minute. Indeed, references to the tables will show that we can hardly be sure of any decrease from 2 to 8 minutes with the few observations we have. The heightened form of neural dispositions, due to immediate intense activity, drops quickly to a low level, from which the decrease is comparatively slow. The attention is in this case turned away completely from the first positions, since the subject's interest is to forget rather than remember them. In 24 hours the subject can sort the cards as rapidly as at first. This, however, does not mean that the neural disposition has vanished. The experiment consists rather in raising a habit temporarily above the rest, and noting its interference with another raised similarly above the same base of opposition. The difference at 3 seconds does not measure the strength of the habit at 3 seconds from the time of sorting the first pack, but is relatively too small, since the habit is found to decrease rapidly during the process of sorting the second pack. A considerably weaker habit applied uniformly as an interference might have produced the difference. Only an estimate can be made as to how many seconds of work with the second pack would have had to elapse before this average habit would have been reached. If we put it at 30 for the 3-second interval, it would be nearly 45 for the 15-second interval; and the decrease in the differences would be between these two points. But after about a minute and a half, when the great decrease in the differences is at an end, the method measures accurately the rate of forgetting.

A tentative analysis of the process may be permitted here, as it will bring out some of the essential conditions of the experiment. In sorting the first pack, two of the subjects learned the positions during the placing of about a third of the eighty cards, so that the process could go on without further use of the eye to find the place for, or to verify, each throw. After a little practice there was very little feeling of effort, and the process was largely mechanical. It takes on the average all the subjects when in practice 65 seconds to sort the first pack; to sort the second immediately afterwards requires, however, about 85 seconds, and numerous false motions in the direction of the first positions are made. The false movements, and the errors which the subject was obliged to correct, and the consequent retardation, show that a strong association has been formed. At first the eye is moved in the direction any card is to occupy. The movement of the arm in placing the card may follow the eye as a reflex or as a separate act of the will. In the first case, in which we have supposed the arm to follow the movement of the eye reflexly, the association which

interferes would be between the visual centres receiving the image of the picture and the centres moving the eye. As soon as the picture was seen, the eye would snap in the direction of the strongest habit, or toward the former positions, and the arm would follow and make the visible false motion, not because of an association between its centres and the visual centre, but because it is in reflex *rapport* with the eye. In the second case, the association would be formed between the image of the picture and the arm movement, and the eye movement might be unimportant in the production of the interference; or the picture may be the signal for sending an impulse both to the eye and the arm at the same time. The localization of the interference is an important psychological question. It is a mechanical struggle of habits, and might at first be supposed to be very definitely localized, so that the association of the positions with the motor organs of speech would not interfere with sorting by the hand. In nine experiments, in which the positions were learned from seeing the cards on the table by repetitions, like those employed by Ebbinghaus with the nonsense syllables, an interference of 30.02 seconds took place, as compared with sorting a pack, without such learning, immediately before and after each series of experiments. In another series the positions were learned, not by looking at the cards, but by being told where each one was, till they were learned. It was supposed that by using neither the eye nor the arm, but getting the information by another sense, and expressing it with the vocal organs, interference might be avoided for the sorting when the eye and arm were the organs used. The interference in this case was 23.25 seconds, as the average of nine experiments. The positions were learned in each case, till they could be repeated forwards and backwards. The interference is considerably less when the ear was the receiving sense, but the experiments are too few to make definite comparisons. The whole matter requires additional experimental study. As far as these experiments go, they show that it makes little difference what sense is the receiving sense, or what organ the reacting organ, as regards the production of interference for the sorting by hand. The information gained by one sense is perhaps not confined to the centres of that sense, but awakens the other senses whose special data have contributed, or may contribute, to the perception of the object—the eye visualizing, the arm moving, or tending to move, as if it were the organ used.

In sorting the cards quickly, the mental process is not an intense form of the process that would take place if the sorting were done slowly. In the latter case no false motions

need be made, since the subject would know that no position of the first stage of the experiment was repeated in the second, and if he made a mistake, he would learn from experience, and not repeat it. Only a very small part of such complete consciousness is present in rapid sorting, since not only are errors made once, but the same error is repeated many times, especially with the shorter intervals. When the effort is to make the greatest possible speed, we produce the condition of automatism voluntarily. We have in this a laboratory method of studying habits.

An illustration of interference is found in the familiar game in which a person is asked to repeat sentences like the following as fast as he can: "Shall he sell sea shells? Shall she sell sea shells?" "Shoes and socks shock Susan," or, "Peter Piper picked a peck of pickled peppers," etc. Great difficulty is experienced. In the first half of the first sentence the simple *s* sound is repeated twice in "sell" and "sea;" it begins "shells," and if the person repeats very rapidly, so as to exclude the higher mental processes, the habit formed by simply repeating the *s* sound twice will make him say "sells" for "shells." Similarly the repetition of *sh* twice in the second half will probably make him say, "shall she shell." Every one is familiar with similar errors in writing, where words beginning with the same letters are liable to confusion. This is especially noticeable in fatigue, or cases of abstraction and haste. The cards provide an easy means of variations, so that the phenomena can be studied quantitatively. After considerable practice, the visible number of false moves diminishes, but as will be seen in the tables, the average retardation does not grow very much less. A habit of hesitation has been formed, but does not lessen the interference very much, as measured by the increase in time.

Historical.

An extended account of the experiments of others on memory is not needed here, Dr. Burnham's study in the second volume of this *Journal* being sufficiently accessible. A few papers, however, have been published since Dr. Burnham wrote,[1] and some of them have more or less relation to the experiments of this paper.

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[1] Paneth: Versuche über den Zeitlichen Verlauf des Gedächtnissbildes. Centralblatt f. Physiol., Bd. IV., No. 3, 1890.

Schumann: Ueber das Gedächtniss für Komplexe regelmässig aufeinander folgender, gleicher Schalleindrücke. Zeitschr. f. Psychol., Bd. I., 1890.

Schumann: Ueber Contrasterscheinungen in Folge von Einstel-

Müller and Schumann give an account of some interesting and important observations on what they call motor adjustments. If a heavy weight is lifted a number of times, a habit of expending a given amount of energy is formed, and lighter weights lifted afterwards will appear lighter than they ordinarily do. The method of experimenting is first to find a comparison weight heavier than that which can just be distinguished from the standard each time; then the heavy weight is lifted in place of the comparison weight several times. On returning to the comparison weight, it can no longer be distinguished, and may even be judged lighter than the standard. The influence of the motor adjustment was noticed even after twenty-four hours. The phenomenon is thought to be due, not to contrast, but to the formation of the motor habit, and the illusion takes place because the comparison weight is lifted with greater velocity than usual. Dr. Schumann has given an account of similar phenomena in another field. The normal rate of letters on a revolving drum seemed slower or faster than usual, after a short habituation to faster or slower velocities respectively. Vigor and fatigue seem to have a somewhat similar effect. Distances traced by the hand were misjudged after habituation to other distances. These phenomena, which resemble the "interference" of this paper, are very important for psycho-physical experiments as well as for a study of the nervous system.

Dr. Münsterberg has studied the interesting theoretical question whether a habit associated with a given sensory stimulus can continue automatically, while some effect of a previous and different habit associated with the same stimulus remains. He believes the question can only be studied when the attention is entirely distracted from the experiments, and therefore it is to be studied in the actions of daily life, not in the laboratory. The experiments were made with his inkstand, his watch, and the doors of his room. In each case a given habit is practiced (in the course of daily life—not specially) till it becomes automatic—as, for example, taking his watch out of his pocket on the left side. A different habit is then practiced; in the case of the watch, of taking it out of the

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lung. Nachrichten v. d. k. Ges. d. Wiss. zu Göttingen, Dec. 3, 1889. No. 20.

Müller and Schumann: Ueber die psychologischen Grundlagender Vergleichung gehobener Gewichte. Pflüger's Archiv, Bd. XLV., 1889, pp. 37-112.

Münsterberg: Gedächtnisstudien. Beiträge, Heft 4, 1892.

Cattell and Fullerton: On the Perception of Small Differences. Publications of the Univ. of Penna. Philos. Series, No. 2, 1892, pp. 147-149.

pocket on the right side till this is done automatically. He then returns to the old habit, and finds that it takes less time to relearn this than it did to learn the second, and therefore concludes that some effect of it remained, although the second was automatic. If this process is repeated several times with the same object, the time required for relearning each of the two habits grows less and less. This shows, he believes, that the habits are gradually being developed to their maximum strength, and that only a little difference one way or the other is sufficient to make the habit do its work automatically, and that nerve currents do not behave like electric currents, which divide into several conductors inversely as the resistance.

It only remains for me to express my obligation to those that have assisted me as subjects, and in particular to my wife, whose work both as a subject and assistant has contributed much to the success of the research.

A NEW INSTRUMENT FOR WEBER'S LAW; WITH INDICATIONS OF A LAW OF SENSE MEMORY.

By James H. Leuba.

The three methods generally used in the study of the relation between physical stimuli and the sensations they produce—the Method of Least Observable Difference; the Method of Average Error; the Method of Right and Wrong Cases—are only different ways of reaching the same end, viz., the determination of the least observable difference in sensations.

Delbœuf and Wundt have attempted to verify Weber's Law by following another road. Instead of seeking to measure the least preceptible increment of sensation at different points of the scale of intensities, they seek to determine a middle sensation between two others, varied at will. This Method of Equal Intervals *(Methode der mittleren Abstufungen)* admits of a modification of some importance; it consists in the classification between two limits of a series of sensations into groups, in such a way that each group will appear to be at an equal interval from the one immediately preceding, and from the one immediately following it.

The comparison of the magnitudes of the stars, as determined by the eye, and recorded in the various catalogues, with the photometric measurements of their light intensities, is an application of this method, if, indeed, we can assume that the sole desire of the star-catalogue makers was to classify the stars into magnitudes, each appearing equally distant from the next.[1]

This method is evidently capable of wide and useful application, equally well in the measurement of intensities as in that of extensity. Prof. James, contrasting the Method of Equal Intervals, of which the Classification Method is a species, with the three usual methods, says: "At first sight there seems to be no direct logical connection between this method and the preceding ones. By them we compare equally

[1]The last comparison of this kind was made by Prof. Jastrow. See *Amer. Journal of Psychology*, Vol. I. p. 112.

perceptible increments of stimulus in different regions of the latter's scale, but by the fourth method we compare increments, which strike us as equally big. But what we can but just notice as an increment, need not appear always of the same bigness after it is noticed. On the contrary, it will appear much bigger when we are dealing with stimuli that are already large."

If the relation of the sensations to the stimuli producing them is found by the Method of Equal Interval (or the Classification Method) to be the same as when established by one of the Methods of the Least Observable Difference, the just perceptible increment in sensations will have been proved to remain equally big in the observer's consciousness. This question is in itself of sufficient interest to warrant experimentation with the Interval Methods.

Prof. Jastrow is, so far as we know, the only person who has made use of the Classification Method. He applied it in the study of star-magnitudes, just mentioned, and in experiments on the spatial relations of vision, on the tactile-motor sensations, on the time-sense, and on the motor-sense.[1]

In order to adapt this psycho-physic method to demonstrational purposes, Dr. Sanford last year devised an instrument (Fig. 1) for the production and the measurement of artificial stars of different magnitudes, using the principle of the episkotister for regulating the amount of light passing through a minute hole in a metal plate.

The present paper records the experiments made with this device, according to the Classification Method, to test the possible usefulness of the instrument.

Two lots of results were obtained, one with the apparatus just mentioned (Fig. 1), the other with a modification of it, suggested by the writer (Fig. 2).

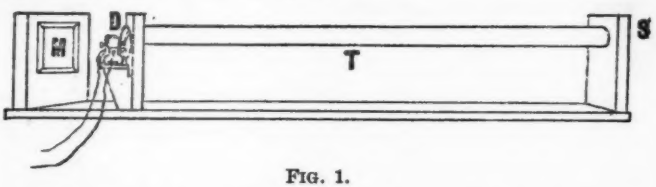

FIG. 1.

The most important part of the first apparatus is the episkotister, two equal discs (D), four inches in diameter, set on the axis of a small electric motor. Each of them has two openings of 90°, in opposite quadrants. The discs can be so placed in relation to each other that their openings will coincide on

[1] See *American Journal of Psychology*, III. and IV.

any angle between 0° and 90°. To permit of exact adjustments, one of them is graduated along one of its openings. A light thrown on the mirror M, is reflected towards the discs, and as they rotate, a quantity of light proportional to the opening passes through to a very small hole, bored in metal, at the extremity of the tube T, and covered over on both sides by pieces of glass to keep it free from dust. The subject, seated at the other end of the tube and looking in at S, perceives the light in the form of a star. The position of the observer can be fixed by reducing the opening of the tube so that he will have to sit with one eye close to the small space left free in order to see the light. The tube T is about six feet long. By means of a switch, the motor can be readily stopped for the readjustment of the discs.

The two extreme stars used as standards were made by an opening of 180° and 10° respectively. The latter is barely perceptible; the former is about the size of the smallest stars of the first magnitude. At the beginning of a new sitting, the subject was shown the two standards, and also during its course, whenever he desired to see them again, though this was rarely asked for. In each series of experiments the subject was shown the forty stars made by the forty openings, between 10° and 180°, which are given in degrees in the degree columns of Table I. The series was so chosen that about the same number of stars would fall into each of the five classes if the psycho-physic law were followed. They were shown in an irregular order, and the subject was requested to group them in five classes, or magnitudes, endeavoring to make the differences between the classes equal. His answers were recorded in tables similar to Table I., opposite the figures in the first column representing the measure of the intensities of the lights. The numerals stand for the class to which the star was referred.

TABLE I.

Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	fifth Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	5th Series.
180	1	1	1	1	1	112	2	2	2	1	2	64	2	3	2	3	2	32	3	3	4	4	3	16	3	4	4	5	5
170	1	1	2	1	1	106	2	2	2	1	2	60	2	3	3	2	2	30	4	3	4	4	3	16	4	3	5	5	3
160	1	1	1	1	1	100	3	1	1	1	1	56	3	2	2	3	2	28	4	4	3	3	5	14	4	4	4	4	3
152	2	1	1	1	1	94	1	2	2	1	1	52	2	1	2	3	2	26	4	3	4	4	4	14	5	4	5	5	5
144	1	2	1	2	1	88	3	2	2	3	3	48	4	3	3	3	2	24	3	3	3	4	5	12	5	5	5	5	5
136	1	1	1	1	1	82	1	2	2	1	1	44	2	2	2	2	4	22	4	4	5	5	2	12	4	5	5	5	5
128	2	1	1	1	1	76	2	3	2	3	1	40	3	2	3	5	3	20	3	4	3	4	4	10	5	5	5	5	5
120	2	1	2	2	1	70	3	3	2	1	2	36	3	2	3	2	3	18	4	4	3	4	4	10	5	5	5	5	5

Table II. summarizes Table I., giving the average of each class in each of the five series, and, at the bottom, the average ratios between the classes. The subject has attempted to place his sensations in an arithmetical series of five terms; we have here the objective measurement of the stimuli which produced these sensations; if Weber's Law were exactly followed, the ratios should be equal.

TABLE II.—CLASSES.

SERIES.	I.	II.	III.	IV.	V.
1st.	138. (7)	91.4 (10)	48.2 (10)	23.7 (9)	11.5 (4)
2d.	133.1 (9)	80.2 (10)	44.6 (10)	18.8 (7)	11. (4)
3d.	142.8 (7)	87.2 (13)	34.2 (8)	26. (4)	13.7 (8)
4th.	125.6 (12)	80.8 (5)	58.8 (7)	23.4 (7)	16.9 (9)
5th.	128.5 (12)	65.5 (9)	36.5 (7)	27.0 (4)	15.7 (8)

	I.—II.	II.—III.	III.—IV.	IV.—V.
RATIOS.	1.66	1.88	1.91	1.75

The class averages given in this table are arithmetical averages. It would have been more correct to follow the indications of the psycho-physic law, and to take the geometrical mean, but as the results obtained by these two methods are very nearly the same, we chose the former to avoid the great labor of calculating the geometrical averages. The numbers in parentheses at the right of the class-averages indicate the number of stars which enter the classes.

Table III. gives the average ratios of all the judgments passed by the nine persons who served as subjects in the first lot of experiments. It involves the classification of 2,120 stars (53 series). The last row of figures represents the final average-ratios of all the results, weighed by the number of series. We shall discuss its significance later.

TABLE III.

CLASSES.	I.—II.	II.—III.	III.—IV.	IV.—V.
Be.—4 s.	1.70	1.94	1.89	1.77
Bo.—12 s.	1.58	1.80	2.18	1.75
Br.—3 s.	1.76	1.96	1.92	1.66
D.—3 s.	1.54	1.97	2.28	1.67
F.—6 s.	1.45	2.21	1.81	1.82
K.—6 s.	1.66	1.90	1.88	1.79
L.—5 s.	1.66	1.88	1.91	1.75
R.—7 s.	1.76	1.87	1.86	1.72
S.—7 s.	1.82	1.99	1.87	1.58
Averages (Weighed).	1.66	1.93	1.96	1.73

At this point the experiments were interrupted by the summer vacation of last year. At the beginning of 1893 Dr. Sanford asked me to complete them.

The falling off of the ratio at both ends of the scale of intensities, for which we could not account (see total average-ratios in Table III.), suggested changing the position of the standards from the extremes (10° and 180°) to a point near the middle of the first and the last classes.

Six series of experiments were taken with the standards at 20° and 160°, and the standards were shown at the beginning and regularly after every five judgments.

TABLE IV.

CLASSES.	I.—II.	II.—III.	III.—VI.	IV.—V.
Leu.—3 s.	1.41	1.76	2.10	1.99
S.—3 s.	1.49	1.45	2.11	1.93
Averages.	1.45	1.61	2.12	1.96

The results (Table IV.) are not based on a sufficient number of experiments to afford a sure basis of comparison with Table III., and in addition to this source of uncertainty, the acquaintance of the two subjects with the scheme of classification had a biasing influence, for in one case, for instance, the subject observed that he was attempting to avoid a mistake which he knew he had made in a previous series, thus judging no more solely from his sensations. The knowledge of the position of the standards in the scale of intensities had also a disturbing effect. The order in which the stars were presented was observed to influence the results. If many stars of about the same intensity were shown successively, the subject would lose sight of the true extent of the scale, and consequently make false judgments. This source of error could not be very great when the standards were shown frequently. The results of these six series do not differ greatly, however, from those of Table III.

The absence of the standards appeared to me the source of a considerable error. During the intervals between their appearance, the subject had in mind a representation of them, unavoidably erroneous in some degree. Even when they were shown regularly at relatively short intervals, as in the last experiments, it often happened that the observer would say, when looking at one of the standards: "I see that I was wrong in my last judgment," thus plainly indicating that he had forgotten the magnitude of the standards. This source of error would naturally bear specially on the last class.

To obviate this defect and to make the conditions of the experiment more nearly those of real star classifications, a new apparatus was made (Fig. 2), in which the standards would

constantly be in sight. The star-making device was also modified in order to allow of the production of a greater number of stars between the limits. A disc (D) seventeen inches

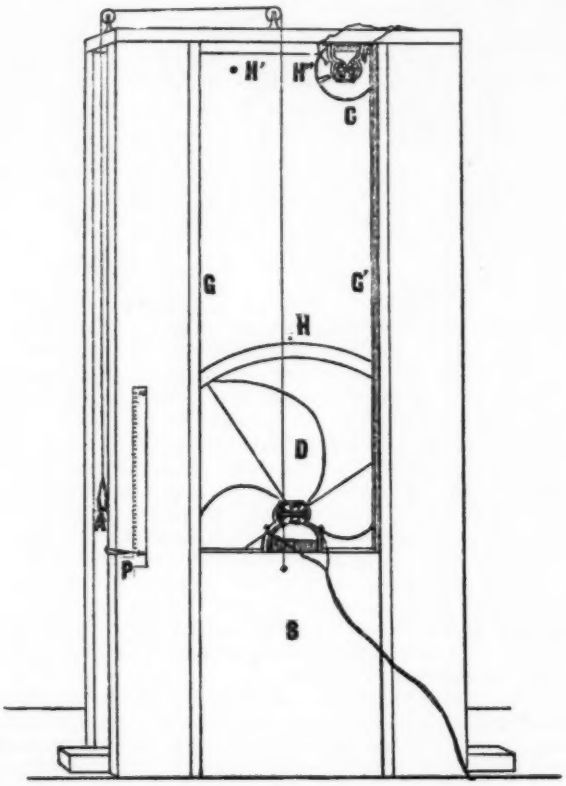

FIG. 2.

in diameter was so cut that as the centre is approached the amount of filled space increases. (See Fig. 3). It is rotated by a small electric motor, fixed on a slide (S). The operator, by means of the handle (A), moves the slide, and with it the disc (D) between the guides (G G'). The position of the disc with reference to the pin-hole (H), through which the

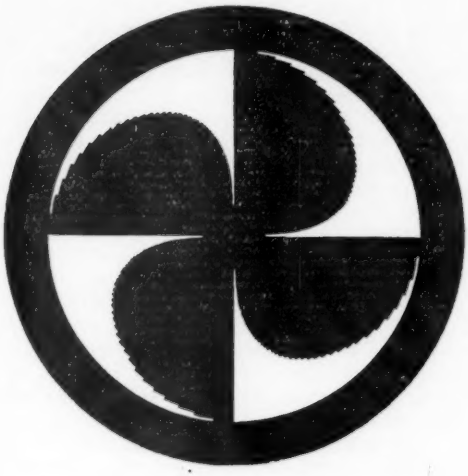

FIG. 3.

light passes, is indicated by the pointer (P), which moves with the slide.[1] The disc can thus be held in fifty different positions, corresponding to the fifty openings expressed in degrees in the figures in the degree columns of Table V. The drawing gives a back view of the apparatus. The subject sat in front, at a distance of about two metres, in a position fixed by a head-rest.

Two other pin-holes (H') and (H''), the latter hidden by the rotating disc (C), equal in size with (H), give the two standards: (H'), the superior standard, by the free access of the light, and (H''), the inferior, by means of a slit in the disc (C). The light is so placed that it falls equally on the three holes, but before reaching them it passes through a piece of oiled paper, stretched at a little distance from them. Kerosene

[1] It is essential to accuracy that the disc should revolve very close to the hole, especially if the light is not in the same horizontal plane with it. When the disc is at some distance from the hole, and the light comes from above, it may reach the hole through a division of the disc above the one recorded as the measure of the magnitude of the star. A slight error, amounting to a notch of our disc, thus found its way into this second series of experiments. As their purpose, however, was chiefly to show the adaptability of the method to general laboratory use, and as the constant error introduced does not interfere at all with the very general inferences drawn from them, it has seemed best to let them pass.

lamps were used as the steadiest light available. The experiments were conducted in a dark room, the light of the lamps being projected in the direction of the apparatus only.

The instructions to the subject were different from those in the previous experiments. Again he was to classify into five groups at equal intervals from each other, but the standards were now used as limits only, and were not to enter into the classes. When a star appeared to be equal to one of the standards, the subject was to designate it as the Superior or Inferior Limit. He had thus before him a sharply bounded scale of intensities.

This apparatus could also be used for the Method of Equal Intervals. The magnitude of star (H″) can be altered at will by changing the episkotister rotating in front of the hole, as explained in the description of Apparatus I. The subject's task then becomes to find a star at an equal distance in intensity from (H′) and (H″).

Before going further, let me insert here a few observations of some importance in conducting the experiments:

Contrast. In order to avoid contrast effects, the lights used as standards should not be too near the star to be classified. In our apparatus they were at a distance of about fifty cm.

After-Images. The after-images of the standards have no doubt, in some degree, a disturbing influence. Fixating the lights longer than necessary should be avoided. A mere glance, repeated three or four times at short intervals when necessary, was found to yield the best results. In this manner the judgment was often made in memory, when the stars to be classified, as well as the standard, were no more before the eyes. The eyes should be closed between the judgments.

Fatigue varied greatly with the person and with the circumstances. But the irregularities resulting from fatigue were always great enough to render worthless observations taken without regard to it. If comparisons were made in rapid succession for a few minutes, the subject lost confidence in his classifications, and his judgments became clearly erroneous. He would place the same star, seen at a very short interval, in widely different parts of the scale of intensities. For instance, the same star was located now in the second class, and now in the fourth.[1]

A rest of two or three minutes reëstablishes certainty of judgment. When in doubt as to the proper place of a star, the longer one gazes at it continuously, the more confused one becomes. To avoid the effects of fatigue, the stars should

[1] After-images may play an important rôle in this confusion.

be shown at intervals, varying with the circumstances, and the series of fifty should generally be cut in two, a rest of some half hour being allowed between the two halves.

The results of the first series at least should be rejected. The subject does not realize at the beginning the extent of the scale which he is to divide into five, and, as he advances in the classification of his first series, he very generally perceives that he has made too large or too small a place for one class to the detriment of the others.

The ratios of subject A., in Table VII., decrease with the magnitude, while those of all the other subjects, whose observations are recorded in Table VII., increase with the decrease of the magnitude. The subject stated that knowing the decrease in the relative discriminative power near the limit of visibility, he modified his judgments accordingly, in order to make the five classes differ from each other by the same quantity. This interference with the sensations accounts sufficiently for the inversion of the common order. The ratios without the judgments of this subject are given at the foot of Table VII.

Extension and Color. The stars appeared to be of different extensions, although the holes were of the same size. The brightest star was judged to cover a surface about ten times larger than the one occupied by the faintest. Some of the subjects noticed that their classification was somewhat modified by this extension element. Irradiation, and, perhaps, an illusion of greater surface, produced by a greater intensity of light, may be the cause of this phenomenon.[1] With the kerosene lamps the color of the stars took a yellowish tint as the magnitude increased. This change of color, although very slight, may have influenced the classification.

Let us now take up the group of results obtained with Apparatus II., and compare them with the first group.

Table V. shows the classification of four series, as made by subject Bo.; Table VI. gives the averages of each class in each of these four series, and, at the foot, the ratios. Finally, in Table VII. will be found the class-ratios, based on 1,100 judgments (22 series), made by five persons. At the foot of Table VII. are the general averages weighted, as before.[2]

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[1] A certain complication of conditions is thus introduced, for, the relation of intensity of stimulus to sensation and of the quality of stimulus to the same, are not identical, but in this the classification of artificial stars stands on the same footing as that of real stars.

[2] In two series of subject Be. (Table VII.), who very soon showed the signs of fatigue, the judgments were taken only at the critical points between magnitudes.

TABLE V.

Subject Bo.

Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.	Degrees.	1st Series.	2d Series.	3d Series.	4th Series.
360	2	2	1	2	216	2	3	3	3	129	3	3	3	4	77	4	4	4	4	46	5	5	5	5	28	5	6	6	6	16.5	6	6	6	6
334	2	2	2	2	200	2	3	2	3	120	3	4	3	4	72	5	4	4	4	43	5	5	5	5	26	6	6	6	6	15.5	6	6	6	6
311	1	2	2	2	186	3	3	3	3	112	4	4	3	4	67	4	5	4	4	40	5	5	5	5	24	6	6	6	6	14.5	6	6	6	6
289	2	2	2	2	173	3	3	3	3	104	3	4	4	4	62	5	5	4	5	37.5	5	5	5	5	22	6	6	6	6	13.5	6	6	6	7
269	2	2	2	2	161	3	3	3	3	96	4	4	4	4	58	4	5	5	5	35	6	6	5	6	20.5	6	6	6	6	12.5	7	6	6	6
250	2	2	2	2	150	3	4	3	3	89	4	4	4	4	54	5	5	5	5	32.5	5	6	5	5	19	6	6	6	6	11.5	7	7	7	6
232	2	3	2	2	139	3	3	3	4	83	4	4	4	4	50	5	5	5	5	30	5	6	6	6	17.5	6	6	6	6	10.75	7	7	6	7
																													10	7	7	7	7

TABLE VI.

SERIES.	Superior Limit.	I.	II.	III.	IV.	V.	Inferior Limit.
1st.	311 (1)	268.8 (8)	145.5 (8)	83.1 (7)	45. (11)	21.3 (11)	11.2 (4)
2d.		302.2 (6)	179.5 (8)	100.4 (9)	50.9 (9)	21.5 (15)	10.7 (3)
3d.	360 (1)	269.3 (7)	154. (9)	81.3 (8)	44. (9)	19.3 (14)	11.3 (2)
4th.		292.1 (7)	181. (6)	98.9 (11)	47. (9)	20.9 (14)	11.4 (3)
Averages.		283.1	165	90.9	46.7	20.8	11.2
Ratios.			1.72	1.82	1.94	2.25	

TABLE VII.

CLASSES.	I.—II.	II.—III.	III.—IV.	IV.—V.
A.—4 s.	2.06	2.04	2.05	1.73
Be.—4 s.	1.85	1.77	2.01	1.93
Bo.—4 s.	1.72	1.82	1.94	2.25
Leu.—6 s.	1.75	1.82	1.91	2.36
T.—4 s.	1.84	1.81	1.70	2.02
Averages (Weighted).	1.83	1.85	1.92	2.09
Averages Omitting Series of A.				
	1.78	1.81	1.89	2.16

The reader will notice that in the general average the ratio of the average intensity of each class, to that above it, increases as the brightness of the stars decreases, and indications of the same thing appear in some of the individual records of both groups. It is greatest between the classes made up of the faintest stars, and it is smallest between Classes I. and II., where the magnitude is greatest. Since the lower limit of our scale of intensities was a barely perceptible light, and the upper limit star much below the superior light intensity, our results agree well, as far as comparison can be made with those obtained by experimenters who used the Methods of Least Differences.

Prof. Jastrow, in the articles on star magnitude, mentioned above, reaches an opposite conclusion. He finds that "the law regulating the ratio of light between stars of one magnitude and those of the next above it, is the psychophysic law as formulated by Fechner, with the modification, however, that the ratio in question, instead of being perfectly constant, decreases slightly with the brightness of the star."[1]

Other persons (Wolff, Pierce, etc.) obtained similar results by comparing special star-catalogues with photometric measurements.

[1]The extreme ratios are 2.802 between the second and the first, and 1.876 between the seventh and the sixth magnitudes.

A little surprise at this is natural when it is remembered that all the observations not based on star-catalogues show that the relative discriminative sensibility falls when weak or over-strong lights are compared. (See the experiments of Aubert, Masson, Helmholtz, and König and Brodhun.) Moreover, this deviation from Weber's Law is general, and applies also to other sensations than to those of light. Biedermann and Löw, experimenting with weights between 10 and 500 gr., found that the sensitiveness to pressure rose with the increase of weight from ten to 400 gr. and then fell rapidly.

Inasmuch as the scale of intensity began with the seventh magnitude, that is to say, with stars perceptible only to acute sight, it was to be expected that the discriminative powers would be proportionately less in the lower magnitudes, and that, consequently, the ratios between the classes would decrease with the increase of the light intensity.

This constant and well-defined disagreement between the persons who dealt with lights produced and measured for their purpose, and those who started from the star-catalogues, indicates, perhaps, that the early astronomers were influenced in their star classification by some other consideration than the desire to make each magnitude equally different from the next. As the number of the stars is much greater in the lower magnitudes, the lower classes may have been made narrower in range for practical purposes. This is, indeed, a very plausible explanation; what the early astronomers wanted, first of all, was a convenient grouping of the stars, and, although magnitude was, no doubt, taken as the basis of the classification, it seems highly probable that the great difference in the number of stars belonging to the different magnitudes should have modified it, consciously or unconsciously, in the direction indicated by the comparisons considered. When we remember how strong was a similar tendency in some of our subjects, we are inclined to say that it could not have been otherwise.

Indications of a Law of Sense-Memory. If we now compare the average class-ratios of Table III. with those of Table VII., we shall see that the most striking difference is the fall from 1.96 to 1.73 in the ratio at the lower end of the scale, in the results of the experiments made with the first apparatus. This fall finds a ready explanation in the absence of the standards while the comparisons and the judgments were made. There seems to be a natural tendency in us to shift the sensations held in memory towards the middle of the scale of intensities. It might be conceived to operate somewhat as follows. The image of a recent sensation tends to recall, by

association, the united residual of all the past sensations of the same kind, and in so doing passes over, in some degree, to this sub-conscious resultant impression. Something of this kind has long been recognized as a law of the recollection of extreme emotional states, but it has apparently escaped notice as a law of sense-memory also. Something similar was observed by Mr. Bolton in the experiments reported in the first paper of this series (see p. 304); and the experiments of Dr. Nichols also, (AMER. JOUR. PSYCHOL., IV., 1891–92, pp. 75–79), showing, as they do, that practice upon a given rate of tapping tends to draw other rates of tapping toward the practiced rate, give a certain amount of support to the opinion that memories generally tend toward what has been most frequently experienced before.

The amount of modification of the sensations in memory, on account of this tendency, should vary with the time separating the perception of the sensations from the comparison, and with their position in the scale. Starting from a middle range of intensity with a deviation equal to zero, the tendency should increase as the extremes are approached.

Supposing this to be true, we should expect that the lower standard used with Apparatus I., being a just perceptible star, would assume in the subject's memory a magnitude greater than the real. This very plausible hypothesis received confirmation in the second lot of our experiments, in the fact that the subjects generally designated as equal to the lower limit stars superior to it in intensity, although the standard was always in sight; so that the average limit, as determined by the judgments of the subjects, was 11° 38′ instead of 10°, the measure of the real limit. If the error can amount to nearly one-seventh of the stimulus, when comparison is always possible, that is, when the time elapsing between the perception of the standard and that of the object to be compared is very short, it will not be surprising to find that it rises to about one-fifth when the comparison is made with an intensity of light experienced from a few seconds to five minutes, or even more, before the judgment is passed, as in experiments recorded in Table III.

This displacement of the standard in memory would affect chiefly the last class, but also all the others up to that unknown point or region which we called the middle of the scale of intensities.

This change occurring to the images of sensations, if further experiments prove it to be a universal one, must be taken into account in all experiments involving the successive comparison of sensations; for, whatever their nature, what the subject would have in mind at the time of the comparison

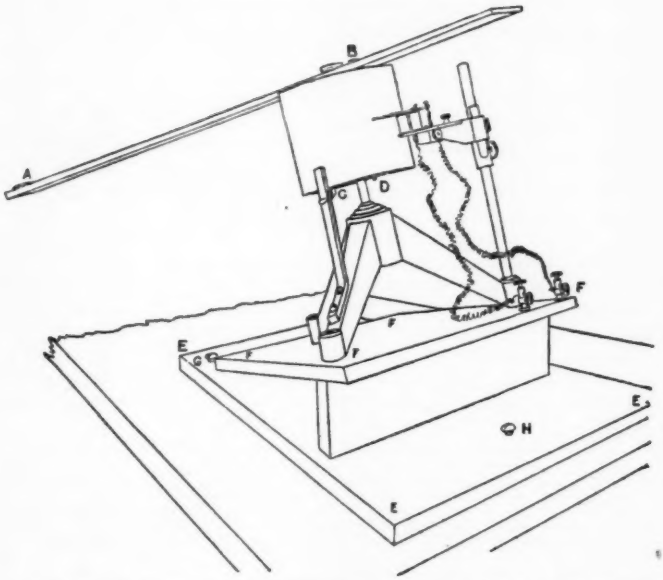
would not be the image of the standard, but another image differing from it in intensity and possibly in form.

Its reality might easily be tested, and, if it proves real, it might be measured by the Method of Right and Wrong Cases (though the experiments would present difficulties), and the construction of the curves representing the displacement in memory of the different species of sensations toward the middle of the scale of intensities, for various lengths of time, would be of considerable value. The influence of this phenomenon on the results of our experiments with Apparatus II. was to decrease the distance, and consequently the ratios, between the classes, as we passed from stronger to weaker stimuli. But as the star-limits were always in sight, the amount of error from this source was comparatively small.

Regarding the results of both groups of experiments from the standpoint of Weber's Law, it is evident that the complete uniformity of ratio required is not shown. The deviations, however, are not extremely great, and the series of magnitudes is very much more nearly a geometrical series than an arithmetical. We feel justified, therefore, in recommending some such apparatus as has been described as a means of introducing star-classification into the laboratory as a psycho-physic experiment.

A NEW PENDULUM CHRONOGRAPH.

By Edmund C. Sanford, Ph. D.



An instrument like that shown in the rude sketch above has been used for two years in the practice work in this laboratory, and as it has proved itself simple and easy to use and tolerably accurate, a description of it may be of interest. In principle the instrument is simply a pendulum swinging completely around in an inclined plane, with a writing surface bent around its axis. This arrangement has two advantages over the ordinary pendulum myograph: the pendulum has no backward swing, and the tracing made by the writing point is a straight line and not a curve.

The instrument was made by fastening a wooden bar upon the top of a "students' drum" of the Cambridge Scientific Instrument Company's pattern, and screwing down the whole on a slightly inclined surface. This bar, lettered *A B* in the cut, has a total length of 1.22 m., a width of 6.4 cm., and a thickness of 1.2 cm. The bar is pierced with a hole 32 cm. from the upper end, which fits snugly about the base of the knob on the top of the drum, and is faced with brass at this point to prevent its wearing loose. The bar is held in place by a thumb-screw (lettered *B*), that passes through into one of the spokes of the drum, and allows the removal of the bar when the drum is to be smoked. A small bob made of two brass plates and weighing 170 grms. is fastened 7.6 cm. from the end of the rod. The drum itself is 13.3 cm. high and 16.1 in diameter, and turns smoothly and easily about a central spindle, a portion of which is visible below it. The inclined base on which it stands is 40.7 cm. long by 38 cm. wide, and makes an angle of 14° with the horizontal base below. In the latter are set three leveling screws, two at the back, one in front, for adjusting it to the horizontal; two of these, lettered *H* and *G*, are shown in the cut.

To secure uniformity in the swings, the pendulum always starts from the same point, *i. e.*, from the catch, lettered *C* in the cut. When the pendulum is ready to fall the peg *D* rests against the catch and the pendulum rod extends upward and to the left. When the pendulum is released, it falls to the left, passes its central point and rises again on the right to a position where it is easily caught with the right hand and carried on again till the peg once more rests against the catch, and everything is ready for another fall.

The time to be measured is inscribed on the smoked paper surface of the drum by a writing magnet. A rack and pinion may be used for raising or lowering this, as shown in the cut, or it may be made to fit tight enough on the post to stay where it is put, as in the instrument in this laboratory.[1]

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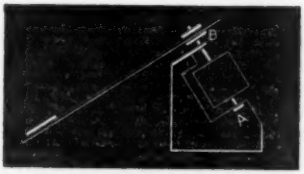
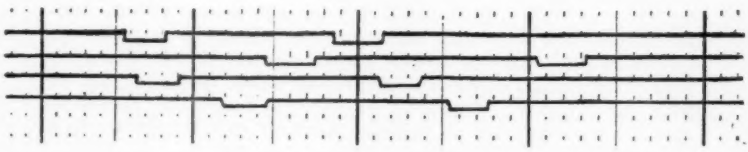
[1] A few tests were made at the start with a still simpler apparatus. This was nothing more than a wooden cylinder, about three inches in diameter, with a steel rod running through it, with a point at the bottom and a collar and nut at the top for attaching the pendulum bar, something as shown in the cut in the margin. The point of the rod rested in a little hollow in a bit of brass at *A*, and was supported by a Y of brass at *B*. The results showed a considerable degree of acuracy.



In order that such an instrument should give accurate results, two things must be regarded: the successive swings of the pendulum must be made in equal times, and the inertia of the writing magnet must be known or compensated. In a recent test of twenty-four successive swings of the pendulum, made just after oiling the spindle, there was a gradual change in the time required for the pendulum to traverse a marked space, corresponding to a little more than a second of time in the middle of its swing, of about .002 sec. Other tests, however, gave less favorable results. In 190 swings, taken ten days later without re-oiling, each tenth swing being timed, the extreme difference reached .009 sec.; but in a third set of 13 consecutive swings, taken on another occasion after re-oiling, it fell again to .003 sec. The difference from set to set, due perhaps to differences in leveling or lubrication, was greater than these, amounting to .018 sec. between the fastest swing measured and the slowest. An error from this cause might perhaps reach one part in fifty between single selected swings, but with care could probably be kept much smaller, and in an average at all events would certainly be so. This error is also distributed over a considerable portion of the swing, and is smaller absolutely when shorter intervals are measured.

The error of the magnet could be measured upon the drum itself by arranging for the making and breaking of an electric circuit at a fixed point by the motion of the drum, but it is simpler to exclude the error by using the same movement of the armature of a single magnet to indicate both the beginning and end of the time to be measured, as indicated in the cut below. This is accomplished without difficulty by put-



ting the beginning and ending keys in parallel circuit and letting the closure of the first be shorter than the time to be measured. In case the presence of a constant error is unimportant, as, for example, in reaction-times, where relative results are aimed at, the use of the magnet may still further be simplified by making the closure of the circuit correspond to the beginning of the time to be measured and its opening

with the end. The error introduced is the difference between the inertia of the magnet at the make and its inertia at the break. If a Deprez signal is used as a time-marker, this difference should not exceed .003 sec. and with careful adjustment would be practically zero.

A defect of such an instrument is that it does not move evenly throughout a swing, but first slowly, then rapidly, and at last slowly again, so that a given distance on the surface of the drum does not everywhere mean the same amount of time. The simplest way to obviate this is to accompany the time tracing with a parallel tuning-fork tracing, but this requires a laborious counting of the waves in the latter. An easier way, when the successive swings are tolerably uniform, as in this instrument, is to take one tuning-fork tracing, make it permanent, and stick it, taking care not to stretch the paper, on the edge of ruler. When this has once been counted and marked off, it can be applied to any time tracing and the amount read off, at once. A still more expeditious way (used already by Dr. Bowditch upon pendulums) is to have the scale printed directly on the paper with which the drum is covered. If the drum is lightly smoked the lines of the scale can be seen through the blackening and the time tracing read at once in units of the scale, and by estimate to a tenth of one of them.[1] In the cut above a portion of such a scale is shown; the finest divisions give hundredths of a second directly, and thousandths by estimate. An intelligent engraver can cut a block for printing a scale of this kind from a tuning-fork tracing taken with a Deprez signal or other time-marker; but he should be cautioned with regard to accuracy, and especially to get the scale lines exactly parallel, otherwise another error is introduced. An error is also apt to creep in in the estimate of tenths, but should not amount to more than one-tenth, so that the total error of any single measurement, provided the scale is exact, ought not to exceed three or four thousandths of a second for short times, and one part in fifty for times of a second or over, a degree of accuracy that is sufficient for all practice work in psychological time measurements and for many kinds of research.

This kind of a chronograph lends itself easily to measurements of any time intervals less than about two and a half seconds; the time of a whole swing is nearly four seconds, but the scale is so crowded at the ends that they are practically useless. Anywhere in the middle second of the swing

[1] When the record has been read the paper need not be removed from the drum, but may be wiped off and resmoked, and this may be repeated a number of times.

the scale is open enough to admit of estimating tenths of a division. All the ordinary reaction- and association-time experiments fall below this limit, and the instrument is useful for that sort of experiment because it is nearly noiseless.

The chronograph can readily be adapted for other time experiments. If two break circuit keys are arranged so that they may be opened by the turning of the drum, Exner's interesting experiments on nearly simultaneous sensations in different senses can be repeated, and the drum itself can be used to measure the time between the stimuli, while at a few minutes' notice these fittings and those for the chronograph can be removed and the drum restored to its original purposes.

If the laboratory already has a drum, writing magnet and electrical tuning-fork, as most with full equipment have, the expense of setting up the chronograph (aside from the block for printing scales, which should cost from 10 to 15 dollars) is not great; if these have to be bought new the instrument will cost between 40 and 50 dollars.

LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

(*Fourth Paper.*)

V.—VISION. (*Continued.*)

Seeing of Light and Color.

The aim of the following experiments is not to adjudicate conflicting color theories, but rather to present the most important experimental facts that all color theories must regard.[1] Authoritative accounts of the theories may be found as follows: Young-Helmholtz theory; Helmholtz, Handbuch der physiologischen Optik,[2] 2te Aufl., pp. 344-350. G[1] 290-294, 320-321, 367; F. 380-387, 424-425, 484. Also Popular Scientific Lectures, 1st Series, New York, 1885, pp. 249-256. Hering's theory; Hering, Zur Lehre vom Lichtsinne, pp. 70–141, (two communications to the Vienna Academy, April 23 and May 15, 1874); Ueber Newton's Gesetz der Farbenmischung, pp. 76-79, a very brief account of his own in connection with a general account of theories. These are the most prominent theories, and something on them, especially on the first, will be found in the physiologies and in some works on the use of color in the arts. Other theories more or less different from these will be found as follows: V. Kries: Die Gesichtsempfindungen und ihre Analyse, Du Bois-Reymond's Archiv, 1882, Supplement-Band, vi, 1-178. Wundt: Physiol. Psychol., 2te Aufl., pp. 453-456; 3te Aufl., 491–496. Also Philos. Studien, IV, 1888, 355-389. Donders: Ueber Farbensysteme, Archiv für Ophthalmologie, XXVII, 1881, H. 1. Noch einmal die Farbensystem, *ibid.*, XXX, 1884, 1. Göller: Die Analyse der Lichtwellen durch das Auge. Du Bois-Reymond's Archiv, 1888. Christine Ladd Franklin, Eine neue Theorie der Lichtempfindungen. Zeit. für Psychol., IV, 1892, 212.

On color vision in general may be mentioned, besides these works of Helmholtz, Hering and Wundt: Fick: Qualität der Lichtempfindungen, Hermann's Handbuch der Physiologie, III, Th. i, pp. 160-232. Maxwell: On the Theory of Compound Colours

[1] For concise statements of these facts, see Wundt, Physiologische Psychologie, I, 487 (cited by Ladd, Phys. Psych., 338), also p. 501, and Christine Ladd Franklin, Zeit. für Psych., IV, 1892, 212.

[2] The second edition of Helmholtz's great work is as yet incomplete. The latest complete edition is the French translation, Optique physiologique, Paris, 1867. To facilitate reference when pages are cited, the numbers are given preceded by G[2] for the second German edition, and by G[1] for the first German edition, and by F. for the French translation. Occasional errors in the pages for G[1] may have crept in, for that edition was not at hand and the pages for it have been taken from the double paging in G[2] and F. The error can hardly amount, however, to more than a page one way or the other.

and the Relation of the Colours of the Spectrum, Phil. Trans. 1860; and On Colour Vision, Proc. Royal Institution of Great Britain, VI; reprinted in Maxwell's Scientific Papers, I, 410-440; II, 267-280. Rood: Students' Text-book of Color, New York, 1881. Aubert: Physiologie der Netzhaut, Breslau, 1865; also Grundzüge der physiologischen Optik, Leipzig, 1876, pp. 479-572. This work forms part of the second volume of Graefe und Saemisch's Handbuch der gesammten Augenheilkunde. Charpentier: La Lumière et les Couleurs, Paris, 1888. Von Bezold: The Theory of Color in its Relation to Art and Art Industry, Boston, 1876. Benson: Manual of the Science of Colour, London, 1871, pp. xii, 58. Chevreul: The Principles of Harmony and Contrast of Colors, London, 1859. Le Conte: Sight, New York, 1881. Ladd: Elements of Physiological Psychology, New York, 1887. Beaunis: Nouveaux Éléments de Physiologie Humaine, Paris, 1888; and other standard physiologies.

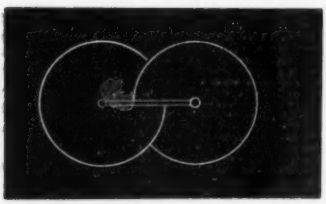
Apparatus. In addition to the blue and red glass, the colored papers and the black and white cardboard used in the previous section, there will be required for this, pieces of yellow, green and violet glass, or of colored gelatine (see below), a small pane of clear glass, a mirror, a sheet of white tissue paper or other semi-transparent paper, and pieces of gray paper or cardboard of different intensities. Gray papers can be made by painting white paper over with India ink; or a tolerable substitute may be made by overlaying black paper or cardboard with one or more thicknesses of tissue paper.

For some of the contrast experiments (Ex. 141 ff.) the gummed parquetry rings and the lentil dots used by the kindergarteners are extremely convenient, and are not expensive. The rings are 1, 1 1-2 and 2 in. in diameter and 1-8 in. broad and are to be had in a considerable variety of colors; the dots are 1-4 in. in diameter, and in six colors. See catalogue of the Milton Bradley Co., Springfield, Mass., pp. 49 and 71.

The standard of color when exactness is important is, of course, the spectrum; and experiments with pure (*i.e.*, monochromatic) spectral colors are the final appeal. The apparatus required for a complete study of color sensations with spectral colors, especially when quantitative results are aimed at, is extremely refined and correspondingly expensive. The spectrophotometer pictured on p. 355 of Helmholtz's Physiologische Optik, 2te Aufl., is quoted by the makers, Franz Schmidt und Haensch, Stallschreiber-Strasse 4, Berlin, S., at mk. 750. Other apparatus of similar purpose ranges from mk. 375 to mk. 3500. The spectrophotometer of the Cambridge Scientific Instrument Co. costs £15. Simple qualitative experiments like those of this section can, however, be made without very expensive apparatus, and for the most part without spectral colors. Where spectral colors are employed a simple prism (costing from 15 cents upward), or at most an ordinary spectroscope, such as is found in every chemical and physical laboratory, will serve amply. A pocket spectroscope even, costing from $5 or $6 upward, will show a good deal, and is useful in determining approximately the composition of light transmitted by colored glass. If nothing more is done, it is desirable that the experimenters familiarize themselves with the appearance of the spectral colors and the chief Fraunhofer lines as landmarks in the spectrum. By combinations of thin sheets of colored gelatine, light that is practically monochromatic can be secured; see a paper by Kirschmann, Ueber die Herstellung monochromatischen Lichtes, Wundt's Philos. Studien, VI, 1891, pp. 543-551. Such sheets are used before calcium lights in the

theatre for the projection of colored lights upon the stage, and may be had of dealers in stereopticons. A. T. Thompson & Co., 13 Tremont Row, Boston, sell red, yellow, green, blue, violet and purple in sheets, 20 1-2x16 3-4 inches, at 25 cents a sheet. For many purposes these sheets are as good or better than colored glass.

For the study of the phenomena of color-mixing with artificial colors, the most satisfactory instrument is the color top or rotation color-mixer in some one or other of its numerous forms. One of these was mentioned in the introduction to the previous section on the Mechanism of the Eye and Vision in General, namely, a little electric motor. All the experiments of this section that require a color-mixer can be made with such a one. Many if not all of them could be made with the color tops sold as toys, or with the very simple one suggested by Dr. Bowditch in his Hints on Teaching Physiology, to wit, a button-mold fitted with a peg and spun with the fingers. One made of a button-mold an inch and three quarters in diameter and carrying disks two and a half inches in diameter, shows the contrast effects of Ex. 142*d* as elegantly as could be desired. The disks are held in place by a piece of rubber tubing of very small bore fitting snugly upon the stem and twisted down upon the disk like a nut. Larger apparatus specially designed for color-mixing may be had of all physical instrument dealers. Among the rest may be mentioned the *Farbenkreisel* made by R. Rothe, Mechaniker des physiologischen Instituts der k. k. Universität, Prag (Wenzelsbad), at mk. 30. A fine instrument by the same maker for rotating a horizontal disk either by foot or hand, with additional parts for studying the color-blindness of the peripheral parts of the eye, costs mk. 160. The color-mixer of the Milton Bradley Co., Springfield, Mass., costs $10, including disks, etc.; the color-mixers of the Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, cost £6-10 and £10. R. Jung, Heidelberg, has rotation apparatus, including one that moves by clock-work at mk. 50-65 with disks. The important thing in such a piece of apparatus is that it should rotate rapidly enough to give a smooth and steady mixture of two colors when these occur but once each upon the disk, *e. g.*, to give an even gray with a disk that has a solid 180° of black and a solid 180° of white. When this is the case the two disks may be slipped together, as in the cut, and any required proportion of the colors easily arranged. If the rotation is not sufficiently rapid the sectors must be made smaller and more numerous, *e. g.*, four sectors of black of 45° each separated by four sectors of white of the same size. This is not difficult when the proportions of color are to remain constant, but where adjustments are to be made the multiplicity of sectors is a disadvantage. Rothe and the Milton Bradley Co. supply colored paper disks in considerable variety evenly cut, and this is an important point, for if the cutting is inexact the disks will appear with bothersome fringes of color when in rotation. The centre hole in Rothe's disks is of course cut to fit the Rothe color mixers. His disks may be had in two sizes, 20 cm. and 11 cm. in diameter, at prices ranging from 60 to 105 kr. per doz. for the large, and 20 to 30 kr. for the small, according to color. Colored papers of excellent color and surface (shiny papers are to be avoided) may be had of R. Jung, Heidelberg.



A rotation apparatus that will serve excellently for class demonstrations is a carpenter's polishing lathe, which is to be obtained at some hardware stores and sells at $4.50. It can be screwed to the table and worked by the hand or foot. Unfortunately it can hardly be made to rotate rapidly enough to blend 180° of white with 180° of black, but for fixed disks with more numerous sectors it works excellently; and though made for so remote a purpose can be used without change and will carry disks up to a foot in diameter. With a very little alteration it would carry them twice as large. Perhaps a maximum of simplicity is reached in the use of a boys' "buzzer" as a color-mixer, which the writer has heard recommended, but never tried. Special disks for use in certain experiments are shown in cuts accompanying them.

In addition to the color-mixer and disks, a stereoscope and stereoscopic diagrams (see cuts accompanying the experiments) and a double refracting prism will be needed. Any stereoscope will answer, but the hood and the central partition should be removed. The double refracting prism may be purchased at no very great expense from dealers in physical instruments.

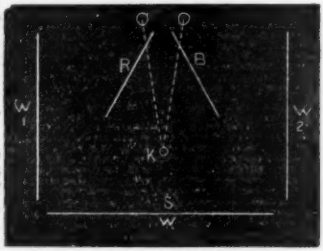
In Ex. 142*b* and 148*b*, a small wooden frame made by fixing two pieces of board 6x6 in. together at right angles, is needed (see diagram accompanying Ex. 142*b*). The convenience of the instrument is much increased if guide strips of wood or metal are attached to the vertical and horizontal pieces, so that the diagrams to be used upon them will be held in place and yet be easily interchangeable. For exhibiting a very deep black in Ex. 130*a*, a black box may be prepared as follows. Make a light tight wooden box 8x10x12 inches in size; cut a two-inch hole in one end and have the whole painted a dull black, both within and without. Before closing it finally, divide it by a slanting partition running obliquely upward and forward from the back edge of the bottom to a point on the top about four inches from the front. The front side of this partition should be covered with black velvet. In comparison with the black that is seen on looking through the hole against this slanting velvet, the gray character of the black paint, of ordinary black cardboard, and even of black velvet, is easily recognized.

The easiest test for color-blindness is made with colored worsteds, which may be had of any dealer in oculists' supplies. An approved selection of colors in sufficient variety is sold by N. D. Whitney & Co., 129 Tremont street, Boston, Mass., at $2.50. A small card of wools for testing color vision is to be found on the inside cover of Galton's Life History Album, London, Macmillan & Co., 1884.

Apparatus is helpful in measuring out the color fields in Ex. 128, though it need not be elaborate. At its simplest two things are necessary: something for steadying the head, and a broad surface perpendicular to the line of sight on which to map out the color fields. A block on which to rest the chin and a convenient wall might do. If something a little more permanent is desired, the head-rest shown in the last section (*American Journal of Psychology*, IV. 1891-92, 474) can be clamped to the front edge of a narrow table, and a screen of light boards (or better, a wooden frame covered with black or gray cloth) fastened about a foot back from it. This distance must not be great, or the screen will need to be of excessive size. It is well to paste a vertical and horizontal scale upon the screen crossing at the point immediately before the eye, so that distances may at once be read off. Such an instrument is known as a *campimeter*. A more perfect instrument for this purpose is the *perimeter*. Of this instrument there are many forms; that of Priestly Smith is perhaps as convenient as any except the most elaborate

ones. In this instrument, not to attempt a detailed description, the place of the screen is supplied by a curved arm that can be turned about an axis at the point on which the eye is fixed, and in turning would describe a hemisphere of which the eye is the centre and the fixation point the pole. The arm is marked for every 5°, and the limits of the field of vision on any meridian can at once be read off and recorded. The record is made by a needle prick in a diagram carried by the instrument, a new diagram being inserted for each eye tested. The price of this instrument from R. Jung, Heidelberg, is mk. 60, from New York dealers $30.

For Ex. 145 and 151 an apparatus devised by Hering and described by him in the *Zeitschrift für Psychologie*, I, 1890, 23-28, is extremely convenient. It is made by R. Rothe of Prag at 28 marks. The apparatus is simple, however, and any carpenter can make of wood one that will answer. The first aim of this instrument is to secure a binocular mixture of blue and red. For that purpose blue and red glasses before the eyes may be used, provided that a good deal of white can also be mixed in with the color of the glass. This is done by letting the glasses stand at an angle before the eyes and reflect on their upper surface the images of suitably placed white screens. The quantity of white light is regulated by the position of the screens with reference to the source of illumination and by the inclination of the colored glasses. The following cut shows diagrammatically what the arrangement of glasses and screens is.



In the cut W_1 and W_2 are the screens just spoken of, R and B the red and blue glasses, W a white surface carrying a narrow black strip at s, and k is the point upon which the eyes are fixed. In an instrument made by a carpenter for the laboratory of Clark University, the following plan was followed; it is here reproduced not because it is the best, but for the sake of definiteness. The stuff used in the instrument was almost all seven-eighths of an inch thick, and that thickness may be understood except where something else is stated. The base is a board 30 in. long, 12 in. wide. In the middle of this is placed another board 12 in. long and 10 in. wide, leaving a margin of an inch on each side and of nine inches at the ends. This little platform bears the white cardboard corresponding to W in the diagram. On the nearer edge of this platform is fastened an upright post 15 in. high, 3 in. wide. At its upper end on the forward side this post carries the frames that hold the glasses R and B. The glasses are 4 in. square, and are framed on three sides only, the upper edge being left free so that the glasses may come close to the eyes. The frames are small pieces of board 6 in. long, 5 in. wide with a square piece (three and three-quarters inches on the side) taken from the middle of their upper ends, leaving them like a U with very square corners and a heavy bottom. Over these square holes the glasses are fastened. The frames are fastened with a single screw each to the post, the screws penetrating the frames about an inch and a half from the free edge of the glass. When in position, the glasses rise about three-quarters of an inch above the top of the post, and stand like the sides of a roof. They do not quite meet, however, but leave a space for the observer's nose between them when the apparatus is in use. The screws that hold

the frames should be tight enough to hold the frames in position, but not so tight as to prevent their turning in adjustment. On the front of the post and about six inches upward from its foot is a wire about three and a half inches long, extending forward from the surface of the post and perpendicular to it. At its end is a little button of cork, the fixation point *k* in the diagram. The side screens of the instrument are exactly alike and the description of one will do for both. Each screen is a piece of half-inch board 9 in. wide and 13½ in. long. This board turns midway from top to bottom on a horizontal axis and in a light frame just large enough to enclose it. The frame itself is fastened to a broad piece of board which forms its base and rests in turn on the base board of the instrument. A peg in the middle of the first of these, fitting into a hole in the last, allows the rotation of the frame and screen about a vertical axis. The screen is thus made adjustable in any direction. Its face is covered with white cardboard. The only remaining part of the instrument is the strip of black paper, a quarter of an inch wide, represented by *s* in the diagram, which is pasted on *W* perpendicular to the post. It is highly important that *W* be without speck or spot, and that the colored glasses be as free from flaws as possible. The instrument as described is intended for binocular contrast. For binocular color-mixing, other pieces of glass besides red and blue are needed for other combinations and the black strip is not required.

Another simple demonstrational instrument of Hering's contriving is for the study of changes of brightness in colors and can also be adapted for contrast. Its principle is the same as that used in the side screens in the last instrument, namely, change of position with reference to the source of illumination. A white card, provided that its surface is not shiny, receives a maximum of light and looks brightest when it stands perpendicular to the light. As it is turned and the light falls obliquely upon it, it receives less and less and looks darker and darker. If it is shiny, as most paper and cardboard are, this change is not uniform, but this does not much interfere in this instance. The paper used should, however, be dull finished. The instrument at its simplest is a tall box open in front and with a hole in the top to look through. It is painted black inside and contains a screen that can be turned about a horizontal axis, and thus receive light perpendicularly or obliquely as desired. It is, however, convenient to have a frame instead of a permanent screen so that a number of cards of different color or brightness can be interchanged, and to have the box double so that two frames can be used side by side and comparative tests can be made. When contrast is to be introduced, a second pair of frames above the first and high enough so as not to shade them are introduced. The cards that are used in these upper frames must each be pierced with a hole, say 2x4 cm., near the middle, in such a position that when the eye looks through them from the top of the box, nothing but the card in the frame below can be seen. The hole must be carefully and cleanly cut, and the edge, if it shows white, must be colored like the surface of the card. When such a hole is looked at with a single eye, it is easy to conceive the part of the lower card seen, not to be really below, but a part of the upper card itself. This illusion might be strengthened by the use of a feebly convex lens to exclude exact accommodation. Changes of the inclination of the upper frame (provided there is no reflection from its under surfaces) can produce no real change in the illumination of the lower one, but very striking changes seem to follow such changes of the upper one. This instrument in finished form, though without the additional

frames for contrast, can be had like Hering's other apparatus of his mechanic, R. Rothe, Prag.

Many of the color experiments to follow can be demonstrated before a considerable audience by the use of a projection lantern, and some makers of lanterns have diagrams for contrast colors, etc. Their preparation, however, can offer little difficulty to those familiar with the use of the lantern and with the ordinary forms of the experiments.

A given color sensation may be changed in three ways: in *color-tone*, in *intensity*, and in *saturation*, or to use Maxwell's terms, in *hue*, *shade* and *tint*. Changes in *color-tone* are such as are experienced when the eye runs through the successive colors of the spectrum. Changes in *intensity* are changes in the brightness of the color. Changes in *saturation* are such as are produced by the addition of white; when much white light is added, the color is a little saturated. Changes in intensity and saturation if excessive involve some change of color-tone also. The number of primary colors is various in various theories; red, green and violet (or blue) are selected by the supporters of the Young-Helmholtz theory, red, green, yellow and blue by Hering, Mach and others, while Wundt is indisposed to make any particular ones more original for sensation than the rest.

127. Color-blindness. Holmgren's method. Spread the worsteds on a white cloth in good daylight. Pick out a light green (*i. e.*, a little saturated green) that leans neither toward the blue nor the yellow; lay it by itself and require the person to be tested to pick out and lay beside it all other skeins that are colored like it, not confining himself, however, to exact matches, but taking somewhat darker and lighter shades also, so long as the difference is only in brightness and not in color-tone. Do not tell him to pick out "the greens" nor require him to use or understand color words in any way; simply require the sorting. If he makes errors, putting grays, light browns, salmons or straws[1] with the green, he is color-blind; if he hesitates over the erroneous colors and has considerable difficulty, his color-vision is probably defective, but in a less degree. If the experimentee makes errors, try him further to discover whether he is red-blind or green-blind by asking him to select the colors, including darker and lighter shades, that resemble a purple (near magenta) skein. If he is red-blind, he will err by selecting blues or violets, or both; if he is green-blind, he will select green or gray, or both, or if he chooses any blues and violets, they will be the brightest shades. If he makes no errors in this case after having made them in the previous case, his color-blindness is incomplete. Violet blindness is rare. Complete certainty in the use of even such a simple method as this is not to be expected without a full study of the method and experience in its application.

On color-blindness and methods of testing for it cf. Helmholtz: Op. cit. G[2] 357 372, 456-462; G[1] 294-300, 847-848; F. 388-400. Jeffries: Color-blindness, its dangers and its detection, Boston, 1879 (this work contains a seventeen-page bibliography on color-blindness and kindred topics); also an article on Color-blindness in the Reference Handbook of the Medical Sciences, New York, 1886, II, 241. Rayleigh and others: Report of the Royal Society's Committee on Colour Vision. Proc. Royal Soc., LI, No. 311, July 19, 1892. Hering: Zur Diagnostik des Farbenblindheit. Archiv für Ophthalmologie, XXXVI, 1890, Heft i, 217-233; also Die Untersuchung einseitiger Störungen des Farbensinnes mittels binocularer Farbengleichungen. Arch. f. Ophthal., XXXVI, 1890, H. 3, 1-23. See also a

[1] It is difficult to give the tints accurately in words. The experimenter should consult the colored charts given in the books of Jeffries mentioned in the bibliography.

paper by Hess in the same place, pp. 24-36. Kirschmann: Beiträge zur Kenntniss der Farbenblindheit, Wundt's Philos. Studien, VIII, 1892, Hefte 2 u. 3. Helmholtz, Hering, Kirschmann and others give exact methods for determining the particular colors that are lacking. On differences in the apparent extent of the spectrum in different observers, see Morgan: Animal Life and Intelligence, pp. 280-283.

128. Vision with the peripheral portions of the retina. *a.* Campimetry. Color-blindness is normal on the peripheral portion of the retina. At the very centre the pigment of the yellow spot itself interferes somewhat with the correct perception of mixed colors containing blue (cf. Ex. 110). In a zone immediately surrounding this all colors can be recognized. Outside of this again is a second zone in which blue and yellow alone can be distinguished, and at the outermost parts not even these, all colors appearing black, white or gray. The zones are of course not sharply bounded, but blend into one another, their limits depending on the intensity and area of the colors used. *a.* With the campimetrical apparatus at hand, find at what angles from the centre of vision on the vertical and horizontal meridians of the eye the four principal colors, red, yellow, green and blue, can be recognized; try also for white. Keep the eye steadily fixed on the fixation mark of the instrument and have an assistant slide the color (say a bit of colored paper 5 mm. square pasted near the end of a strip of black cardboard an inch wide) slowly into the field from the outside. It will be well to move the paper slowly to and fro at right angles to the meridian on which the test is made, so as to avoid retinal fatigue. Take a record of the point at which the color can first be given with certainty. Repeat several times and average the results. The size of the colored spot shown should be constant for the different colors, and the background (preferably black) against which the colors are seen should remain the same in all the experiments. *b.* Repeat the tests with a colored square 10 mm. on a side, and notice the earlier recognition of its color as it approaches from the periphery. *c.* Try bringing slowly into the field (best from the nasal side) bits of paper of various color, especially violet, purple, orange, greenish yellow and greenish blue; or better, hold the bit of paper somewhat on the nasal side of the field and turn the eye slowly toward it, beginning at a considerable angle from it. If the paper is held before a background, containing a line along which the eye can approach the paper, the eye will be assisted in making its approach gradual. Observe that on the outer parts of the retina these colors first get their yellow or blue constituents, and only later the red or green, and appear in their true color. If the range of choice is sufficiently large, it may be possible to find a red (inclined toward red purple), a green (inclined toward the blue), which, like pure blue and yellow, change only in saturation and not at all in color tone as they move inward toward the centre of the field. These four colors are the *Urfarben* or primary colors of Hering.

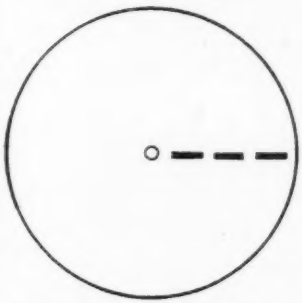
Helmholtz: *Op. cit.*, G2 372-374, F. 399-400. Hess: Ueber den Farbensinn bei indirektem Sehen, Archiv für Ophthalmologie, XXXV, 1889, H. 4. Hering: Ueber die Hypothesen zur Erklärung der peripheren Farbenblindheit, Arch. f. Ophth., XXXV, 1889, H. 4, pp. 63-83; XXXVI, H. 1, 264. Fick: Zur Theorie des Farbensinnes bei indirektem Sehen, Pflüger's Archiv, XLVII, 1890, 274-285. Aubert, Phys. Opt., 539-546.

129. Changes in color tone. With spectral lights, change of vibration rate, if not too small, means change of color tone, but equal changes in vibration rate do not involve equal changes in color tone. The change of color tone is most rapid in the green region of the spectrum, less rapid at the red and violet ends. *a.* With the spectroscope and daylight find the characteristic *D*, *E*, *F* and *H* lines. The *D* line lies in the golden yellow, *E* in the green, *F* in the blue, and *H* at the end of the violet. Between the *D* line and

the *F* line, the vibration rate changes from 526 to 640 billion vibrations per second, and the color runs from yellow, through green to blue, while from *F* to *H*, with the greater change in vibration rate from 640 to 790 billion per second, the change is only from blue to violet. *c.* Notice the tendency of the succession of spectral colors to return upon itself, shown in the resemblance of the red and violet.

Helmholtz: *Op. cit.*, G[2] 289, 320, G[1] 237, F. 319. Rood: *Op. cit.*, 27. Wundt: *Op. cit.*, I, 429. Fick: *Op. cit.*, 173-175, 183. Aubert, Phys. Opt., 530.

130. Changes in intensity. Black and white. Black and white are the extremes of intensity in the series of grays. The ordinary black and white of conversation are considerably short of these extremes. *a.* Compare a bit of black velvet or black cardboard with the black of the black box described above. *b.* Compare ordinary white paper in diffused light with the same in direct sunlight or with a brightly illuminated white cloud. *c.* Just observable differences with medium intensities. Prepare a disk like that shown in the accompanying cut by drawing along a radius of the disk a succession of short lines of equal breadth. Let the breadth of the line correspond to about one degree on the edge of the disk. Since the breadth of the line is everywhere the same, it will occupy a relatively greater portion as it nears the centre. When the disk is set in rapid rotation, each short line will give a faint gray ring, those at the outer edge being very faint, those nearer the centre, darker. Find which is the faintest ring that can be seen, and calculate the proportions of black and white in it.[1] The ratio of the black to the white measures approximately the just observable decrease in intensity below the general brightness of the disk. The results of Helmholtz and Aubert are respectively: Helmholtz, 1:117 to 1:167. Aubert, 1:102 to 1:186, the differences depending on the intensity of the general illumination of the disk. Some wandering of the eyes is helpful, but too rapid motions of the eyes, which tend to break up the even gray of the rings, must be avoided. It is absolutely essential to have the rotation very rapid and perfectly free from vibration — so rapid that with the moderate motions of the eyes, the uniform gray of the rings is not disturbed. If great rapidity is impossible, replace the single black line by two of proportionately less breadth on opposite sides of the disk, or by four at 90°.



Helmholtz: *Op. cit.*, G[2], 384-393, G[1], 310-316, F. 411-419. Aubert: Physiol. Optik., 489-492.

131. Changes in intensity. Colors other than black and white. At their maximum intensity, all colors tend toward white or yellowish white, green becoming first yellow and then white, red progressing hardly beyond the yellow, but blue and violet easily reaching white.

[1] The formula for the amount of black, assuming that the radial line is absolutely black, and taking some arbitrary point of the line, *e. g.*, the middle point, for the calculation, is of course $\frac{b}{2\pi r}$, where b is the breadth of the radial line and r the distance of the chosen point from the centre of the disk.

a. Fix a prism in the sunlight so that it projects an extended spectrum on the wall. Hold a card, pierced with a pin-hole, before the eye, and bring the eye successively into the different colors, looking meanwhile through the pin-hole at the prism. Something of the same kind may be seen by looking through pieces of colored glass at the disk of the sun behind a cloud (in which case the portions of the cloud seen at the sides of the glass afford a means of comparison), or at the image of the sun reflected from an unsilvered glass plate, or by concentrating light from colored glass on white paper with a convex lens. *b.* It is easy to reduce the intensity of colors with the color-mixer by spreading the light of a colored sector over the whole surface of a disk otherwise black.[1] Make a succession of mixtures of red and black on the color-mixer, beginning with a proportion of red that makes a barely observable change, and increase the proportion till the red decidedly predominates in the mixture. Place a smaller disk of black over the larger disks so as to have a standard black in the field. If any of the red shows through either black disk several of the latter should be used together to prevent it. Try also with the other chief colors. Disks like the diagram (in which shaded parts stand for color and solid black for black) show the whole series of such gradations at once, though not quite so satisfactorily. *c.* Carry a number of small slips of colored paper into a darkened room, or look at them through a fine needle hole in a card, and notice the order in which they lose their color quality. *d.* Adjust the spectroscope so that the chief Fraunhofer lines can be seen, and then gradually narrow the slit through which the light enters the instrument. Observe that red, green and violet-blue with a trace of yellow persist longer than the intermediate colors, and that when all the color is gone, there still remains some light in the region of the green. This experiment must be performed in a dark room, or the observer must envelope his head and the ocular of the instrument with opaque cloth. *e.* Purkinje's phenomenon. In a light of moderate brightness, choose a bit of red paper and a bit of blue paper that are about equal in intensity and saturation, or better, make such a pair with the color-mixer by adding white or black till the intensity and saturation appear the same. Carry both into the full sunlight and notice which appears brightest. Carry both into a darkened room, or observe them in deep twilight, or through a very fine needle hole in a card, or even with nearly closed eyes, and again notice which seems brightest. Cf. also Ex. 142*a*.

Helmholtz: *Op. cit.*, G² 402-444; on *a*, 284-285, 322-324, 465, 466; on *b*, 469, 471-472; on *e*, 428-430, 443-444. G¹, 234, 280, 281, 317-321. F. 315, 369, 370, 420-425. Fick: *Op. cit.*, 200-202. Aubert, Phys. Opt., 531-536. Rood: *Op. cit.*, 181-194. For measurements of the just observable differences of intensity for different colors, see Helmholtz: *Op. cit.*, G² 402-416; Aubert, Phys. Opt., 531; Fick: *Op. cit.*, 177, and the references given by them. Benson: *Op. cit.*

132. Changes in saturation. Repeat Ex. 131*b*, using colored sectors or stars on white disks instead of black. If star disks are used, it is best to give the rays of the star a leaf shape, for the smaller quantities of color toward the outer ends of the narrow rays fail to

[1] Since the black of the disk is really a very dark gray, this is not an absolutely pure experiment, but is sufficiently exact for the purpose.

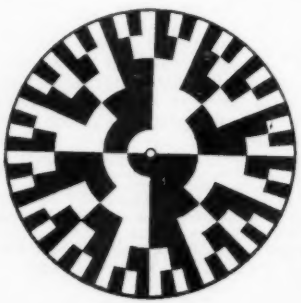
make an impression on the white. Notice the paling of the color when mixed with white and the relatively preponderating effect of the latter. Notice also a tendency to change in color tone as well as in saturation, especially when the amount of color is small. Red tends toward rose, orange toward red, indigo toward violet, blue-green toward blue, etc. According to Rood's experiments with the color-mixer, yellow-green and violet are unchanged; Helmholtz with spectral colors gets somewhat different results.

Helmholtz: *Op. cit.*, G² 322, 476-471, G¹ 281, F. 309. Aubert: Phys. Opt., 531-532. Rood: *Op. cit.*, 194-201.

133. Size of the colored field. The color sensation is not independent of the size of the retinal area stimulated, if the latter is small; and is also affected by the background against which the small colored area is seen. Paste on pieces of black and white cardboard small squares of several kinds of colored paper, one series 5 mm. square, one 2 mm. square and one 1 mm. square. Walk backward from them and notice their loss of color. Observe also the changes in color-tone.

Helmholtz: *Op. cit.*, G² 374-375, G¹ 300, F. 399-400. Aubert: Phys. Opt., 536-539. E. Fick: Notiz über Farbenempfindung, Pflüger's Archiv, XVII, 1878, 152 153.

134. Some Phenomena of Rotating Disks. Talbot-Plateau Law. In several experiments of this section use has been made of rotating disks in studying colors and color combination. All such use depends on the phenomenon of positive after-images (Cf. Ex. 116, *Amer. Jour. Psychol.*, IV, 1891-92, 486). A disturbance set up in the retina does not at once subside, but lasts an instant after the removal of the stimulus. If stimuli follow in sufficiently rapid succession the disturbances are added to one another and fused and the result is the same as though the stimuli had reached the retina simultaneously. The rate of succession necessary to give a uniform sensation is from 25 to 30 per second, the rate depending on the illumination of the disk, the higher rate being required for the greater illumination. When once this uniform sensation has been reached the color and brightness of any given concentric ring of the disk are the same that it would have if all the light reflected from it were evenly distributed over its surface, and no further increase in rapidity produces any effect upon its appearance. This is the Talbot-Plateau law. Rotate a disk like that shown in the cut, increasing the rapidity till the innermost portion gives a uniform gray. When this occurs, the rate of recurrence in the outer ring is 32 times more rapid than in the innermost, and yet no difference in shade is to be seen. To show that the gray is actually of the same brightness that would come from an even distribution of the light reflected from the whole surface of the ring, look at the disk when at rest through a double convex lens held at such a distance from the eye and disk that no distinct image is formed, but the disk looks an even blur of gray. When the disk is put in rapid rotation the gray remains unchanged.



On rotating disks and their phenomena in general cf. Helmholtz, *op. cit.*, G2 480-501, G1 337-355, F. 445-471. On the Talbot-Plateau law cf. Helmholtz, *op. cit.*, G2 482-483, G1 338-340, F. 446-450. Aubert, Phys. Opt., 515-516.

135. Some Phenomena of Rotating Disks. Brücke's experiment. As the disk used in the last experiment is allowed gradually to go slower and slower, there will be observed in one ring after another, beginning with the inner one, just as it loses its uniform character, a notable brightening. The white sectors now have opportunity to produce their full effect upon the retina before they are succeeded and their impression cut off by black sectors.

Helmholtz, *op. cit.*, G2 481-485, G1 338-341, F. 446-450. Aubert, Phys. Opt., 510.

136. Some Phenomena of Rotating Disks. The Münsterberg-Jastrow phenomenon. *a*. When the disk used in the last experiment gives a uniform gray, pass rapidly before it a thin wooden rod or thick wire, and notice that a multitude of shadowy images of the rod will appear on the disk. The number of images is greatest in the portion of the disk having the most frequent interchange of black and white. *b*. Exchange the disk for one carrying two or more colors. Notice the repetition of the phenomenon, and that the colors of the images are the colors (otherwise completely blended) which the disk actually carries. The explanation of the phenomenon is not altogether clear, but the sudden changes of the background against which the rod is seen seem to have an effect not unlike that of a stroboscopic disk or of intermittent illumination, which would show the rod at rest in its successive positions.

Jastrow: A Novel Optical Illusion. *Amer. Jour. Psych.*, IV, 1891-92, 201-208.

137. Some Phenomena of Rotating Disks. Fechner's colors. Rotate the disk used in Ex. 119 or that used in 134, or indeed almost any black and white disk with a less rapidity than that required to give a uniform gray, keeping the eyes from following the motion of the disk, and notice the play of colors on its surface. These vary with the rate of the disk and the intensity of the illumination. The colors may not at once appear, but are not difficult to get with steady and attentive gazing. The colors owe their existence to an analysis of the light of the white sectors, depending, not on the different wave lengths of the colors, as in the case of the prism, but on the differences in the times at which the different primary color sensations reach their maximum in sensation. The intermittent stimulation causes a rise and fall in the intensities of the fundamental sensations, but the instant of greatest excitation is not the same for all.

Helmholtz, *op. cit.*, G2 530-532, G1 380-381, F. 500-502. Aubert, Phys. Opt., 560.

138. Color-mixing. A general law of color-mixing, and one upon which almost all experiments with artificial colors must depend, may be stated as follows: Like appearing colors produce like appearing mixtures.[1] Thus an orange that is mixed from red and yellow spectral lights will produce the same purple when mixed with violet that spectral orange of like intensity would itself produce. The colored papers with which the experiments below are made are very far from simple colors (as can easily be seen by looking at scraps of them on a black background through a prism), yet they produce the same mixtures that spectral colors of equal tone, intensity and saturation would do. Three colors properly selected

1. This law of course has reference to the mixture of colored lights and not to mixtures of pigments upon the palette.

from the whole range serve to produce by their mixtures all the intermediate colors (though generally in less saturation) besides purple and white (*i. e.*, gray). The colors generally selected are red, green and violet. Green cannot be mixed from colors that themselves do not resemble it, *i. e.*, it can be mixed from yellow-green and blue-green, but not from yellow and blue, and not in anything like spectral saturation. *a.* Mix a yellow from red and green on the color-mixer. The yellow produced will be dark, and as a test of its purity should be matched with a mixture of yellow and black made with smaller disks set on above the first. In the same way mix a blue from green and violet that shall match a mixture of blue and black. *b.* From red and violet or blue, mix several shades of purple between violet and purple. *c.* From red, green and violet, mix a gray that shall match a mixture of black and white on the small disk.

For demonstrational purposes the result of mixing two colors in different proportions can be shown on a single disk of the star form (see Ex. 131) by painting the star in one color and the ground of the disk in another (or by pasting colored papers instead of painting), but in either case some trial will be necessary to determine the proper size for the rays.

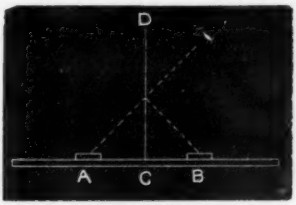
Helmholtz, *op. cit.*, G² 311-316, 320 322, 325-33, 350- 357, 375 376, 473, 485, G¹ 272-277, 279-281, 282, 341, F. 359 365, 367-369, 450. Aubert, Phys. Opt., 521-524, 527-528.

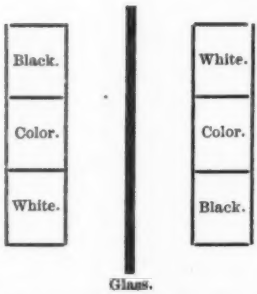
139. Complementary Colors. The combination of red, green and violet mentioned in the last experiment is not the only combination that will give white or gray. For every color there is another or complementary color, which mixed with it will give a colorless combination. Some of these pairs are red and blue-green, yellow and indigo blue, green and purple, blue and orange-yellow, violet and yellow-green. *a.* Try several of these pairs upon the color-mixer, matching the resultant gray carefully with a mixture of black and white on the small disk. It will probably be found in some cases that no possible proportions of the colored papers at hand will give a pure gray. In that case a little of the color complementary to that remaining in the gray must be added. Suppose the red and blue-green papers give, when combined, gray with a tinge of brown (*i.e.*, dark orange), a certain amount of blue or indigo must be added to compensate. For example, with certain papers 180° of blue-green +36° indigo +144° red make a gray that matches 90° white +270° black. To see the true complement of the red used it is then necessary to prepare a disk carrying green and indigo only in the proportions of 180 and 36, *i.e.*, 300° blue-green, 60° indigo. In the same way the complement of the blue-green used is a bluer red than that of the red paper, and may be seen by itself by mixing 288° red with 72° indigo. It is very important here, and in all cases where a resultant white or gray is to be observed, to have some undoubted white or gray in the field to prevent illusions over very faint tinges of color. *b.* Negative after-images when projected on a colorless gray or white surface are seen in colors complementary to those that give rise to the after-images. Compare a pair of complementary colors found in this way with the same pair as found on the color-mixer.

Helmholtz, *op. cit.*, G² 316-319, G¹ 277-278, F. 365-367. Aubert, Phys. Opt., 521-524. Complementary colors can be well seen with polarized light. See picture of the schistoscope, an instrument for showing them by this means in Rood, *op. cit.*, 162; description of Rose's chromatometer (*Farbenmesser*), Helmholtz, *op. cit.*, F 397; also the Leukoskop, Helmholtz, *op. cit.*, G² 368, and references there given.

140. Other Methods of Mixing Colored Lights. *a.* The simplest of these methods is by reflection and transmission. The colors to

be mixed are placed on a horizontal surface on opposite sides of a vertical glass plate. The eye is brought into such a position that the reflected image of the colored field on the eye side appears to overlie the field on the other side seen through the glass. The glass must of course be of good quality and clean. The relative intensity of the colors can be varied by varying their distance from the glass. Bringing the colors near the glass or raising the eye, strengthens the reflected and weakens the transmitted light. Strips of paper placed with their ends next the glass will show an even blending from a mixture in which one predominates to one in which the other predominates, provided the illumination is equal. To mix two colors in exactly equal proportions, arrange them with black and white, as in the diagram below.





Adjust the glass till the grays made by the black and white at the ends exactly match; the colors will then be mixed half and half.

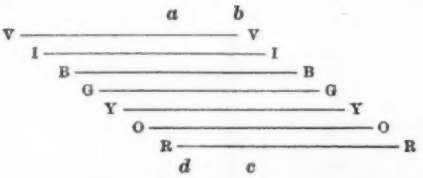
By substituting a bit of glass on a black background for one of the colors and then placing the instrument so that a portion of clear sky may be reflected in the glass, it is possible to mix sky blue with its complement, or with any other color.

b. Colored areas placed side by side can be mixed with the aid of a double refracting prism. The prism doubles both fields and causes a partial overlapping. In the overlapped portion the colors are mixed.

c. Spectral colors can be mixed, though in an inexact way, without more apparatus than a prism and a piece of black cardboard. Fit a piece of black cardboard into the window frame so that it shall cover one pane completely. Cut in the middle of it two narrow slits (1-2 mm. wide and 10 cm. long), meeting each other at right angles and making a broad V. The cuts should be clean and sharp and the slits of uniform width. Look at this V from a distance, 10 or 12 feet, holding the prism vertical. Each arm of the V will give a spectrum, and where they cross, some spots of mixed color may be made out, especially the red and violet giving purple, and red and green giving yellow. The early studies of Helmholtz were

made with apparatus arranged on this principle, but more refined. If lines finer than can conveniently be cut in the cardboard are desired, they can easily be made (after a suggestion of Prof. Pickering) by tracing them on a piece of smoked glass. If the sunlight is allowed to fall on a prism and the spectrum is caught on a white wall or a screen, colors may be mixed with a double refracting prism like the colored fields mentioned above.

d. Something may be done in the way of mixing colored lights with a prism and narrow strips of paper or a diagram like Plate I, or still better, a similar diagram in which black takes the place of white, and *vice versa*. Since a prism refracts different kinds of light to different degrees, it produces a multitude of partially overlapping images of a bright object, which appear to the eye as colored fringes. (Observe through a prism a square inch of white paper on a black background.) These overlapping images may be illustrated by the following diagram, in which the horizontal lines stand for the images and the capital letters for the colors of light producing them.



In the area *a b c d* all the images overlap and the white of the paper is still seen. Toward the left from *a*, however, the different kinds of light gradually fail, beginning with the red. The successive colors from greenish blue to violet result from the mixture of what remains. At the other end a similar falling away of the colors gives the succession from greenish yellow to red. In Fig. 1, Plate I, the spectra seen on the upper and lower edges of the square are brought side by side; on one side red, orange and yellow, and on the other greenish blue, blue and violet. The colors that stand side by side are complementary pairs both in color tone, intensity and saturation; for the greenish blue is the white of the paper less the red, and the blue the same less the red, orange and yellow, and so with the rest; and if the two spectra could be exactly superposed they would make precisely the white from which they originated.

If a very narrow strip of white upon a black ground is looked at through the prism, the images overlap less and another color appears, namely, green, as may be seen in Fig. 2 on the narrow white band between the black bars. When, on the other hand, a narrow black band on a white ground is taken, the spectra of the white surface above and that below partially overlap and give another set of mixtures. If the diagram is held near the prism at first and then gradually withdrawn from it, the advance and mixing of the spectra can easily be followed. Besides the greenish yellow at one end and the greenish blue at the other, there are a rich purple, complementary to the green beside it, and a white between the purple and the greenish yellow. The last is a white produced by the mixture of the blue of one spectrum with the complementary yellow of the other.

In Fig. 3 are shown a number of color mixtures with different proportions of the constituents. In the spectra from the white

PLATE I.

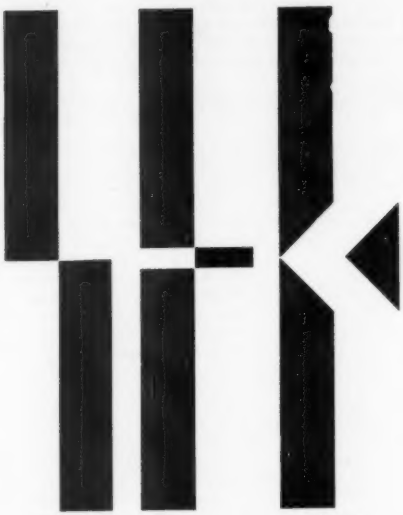


Fig. 1 Fig. 2 Fig. 3

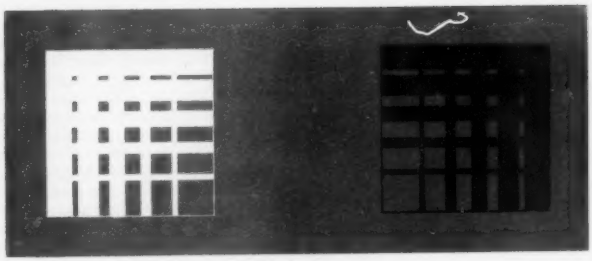


Fig. 4

triangle appear mixtures of each color in the spectrum seen on the white band in Fig. 2, with every other color found there. Upon the black triangle in the spectra from the white edges above and below are seen mixtures similar to those on the black band in the same figure. The diagram should be at such a distance from the prism that a little of the white and black triangles can yet be seen.

On methods of color mixing cf. Helmholtz, *op. cit.*, G² 350 357, 485, 491–493; G¹ 303–306, 341, 346–349; F. 402–407, 450, 457–461. Aubert, Phys. Opt., 521–524. Maxwell, *op. cit.*, On *a* and *d*, Benson, *op. cit.*

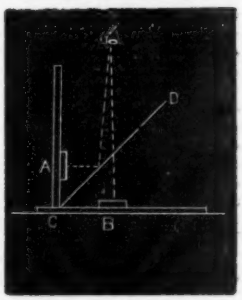
141. Contrast. The effect of one color on another, when not mixed with it, but presented to the eye successively or in adjacent fields, is known as contrast. Two kinds are distinguished, *Successive contrast* and *Simultaneous contrast*. The color that is changed or caused to appear upon a colorless surface, is known as the *induced color*; the color that causes the change is called the *inducing color*. Successive contrast is largely a matter of negative after-images, and their projection upon different backgrounds. Successive contrast: *a*. Prepare a set of colored fields of the principle colors, including white and black, say 3x5 inches in size, and some small bits of the same colors, say 1 cm. square. Lay a small square on the black field, get a strong negative after-image and project it first on the white and then on the other fields. Notice that the color of the after-image spot is that of the field on which it is projected minus the color that produced the spot; *e. g.*, the after-image of red projected on violet looks blue, and on orange looks yellow. Or, to say the same thing in other words, the color of the spot is a mixture of the color of the after-image with the color of the ground upon which it is projected. Thus the blue-green after-image from the red, when mixed with violet, gives blue; when mixed with orange gives yellow. Notice that when the image is projected upon a field of the same color it causes the spot on which it rests to look dull and faded, but when it is projected upon a field of complementary color, it makes the spot richer and more saturated. In general, colors that are complementary or nearly so are helped by contrast, those that resemble each other more nearly are injured in appearance. *b*. These effects in even greater brilliancy can be seen by laying the small square of color directly on the larger colored surface, staring at it a few seconds and then suddenly puffing it away with the breath. Cf. also Ex. 126. *c*. This contrast effect may be so strong as actually to overcome a moderately strong objective color. Place a small piece of opaque orange paper in the middle of a pane of red glass and look through the glass at a clear sky or bright cloud. The strength of the induced blue green will be sufficient to make the orange seem blue. *d*. The contrasting color may even be made to appear upon a surface faintly tinged with inducing color. Rotate one of the disks used in Ex. 132 or Ex. 131*b* rapidly enough to produce an even mixture. Bring the eye within six or eight inches of the disk, stare steadily at the centre for ten or twenty seconds; then suddenly draw back the head. The complementary color will appear to rush in upon the disk from all sides. The explanation is that after the withdrawal of the head the retinal image of the whole of the disk rests upon the part before fatigued by the intensely colored center of the disk.

Helmholtz, *op. cit.*, G² 537–542, G¹ 388–392, F. 510–515.

142. Mixed contrasts. When special precautions are not taken to exclude successive contrast, both kinds co-operate in the general effect. Some of the results are striking and beautiful. *a*. Colored

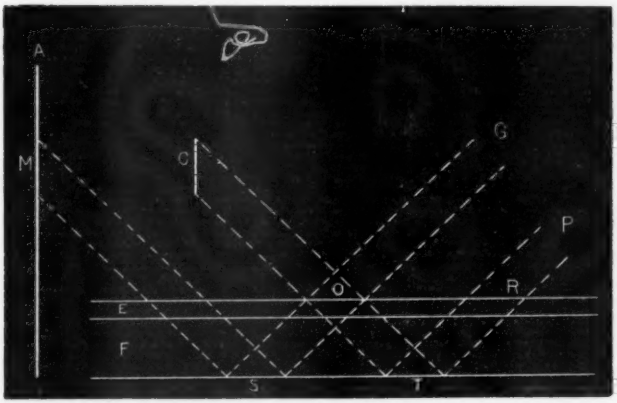
shadows. (1) Arrange two lights so that they shall cast a double shadow of a pencil or small rod upon a white surface, and regulate the brightness (or distance) of the lights so that the shadows shall be about equally dark. The daylight will answer for one light if it is not too strong, but it must come from an overcast sky, for the light from a blue sky is itself blue. Introduce different colored glasses one after another before one of the lights and notice the beautiful complementary color that immediately appears in the shadow belonging to that light. Cf. also Ex. 144. (2) Use a blue glass and adjust the relative intensities of the lights so that the yellow shadow appears at its brightest, and notice that it seems as bright as or brighter than the surrounding blue. As a matter of fact, however, it receives less light than the surrounding portions, for as a shadow it represents the portion of the field from which the light is partly cut off.

b. Mirror contrasts. (1) Ragona Scinà's experiment. Place upon the horizontal and vertical surfaces of the frame described above, white cards carrying black diagrams. Any black spot will answer, but for this experiment diagrams made up of sets of heavy concentric black rings (lines a quarter of an inch wide), separated by white rings of triple width, give an excellent effect. The diameters should be so chosen that a black ring on the horizontal diagram shall correspond to a white one on the vertical and *vice versa*, and shall appear to lie in the midst of the white when the diagrams are combined in the way immediately to be described. A pair of diagrams made up of parallel black bars, a quarter of an inch wide, separated by quarter inch spaces, and so placed in the instrument that they give a checker-board pattern when combined, are useful for keeping in the field a true black with which the changed colors can be compared. The diagrams being in place, hold between the two at an angle of 45° a pane of colored glass, say green, and observe that the black of the horizontal diagram seems tinged with the complementary color, that is, purple. This contrast color may often be improved by slightly altering the inclination of the glass, or by changing the relative illumination of the diagrams by interposing a colorless screen between one or the other of them and the source of light, or by shifting the whole instrument. The mechanism of this experiment will be readily understood after a consideration of the accompanying cut. The glass plate is represented by *C D*, the black portion of the vertical diagram by the projection opposite *A*, that of the horizontal diagram by the projection at *B*. The light reaching the eye from the white portion of the horizontal diagram is colored green by the glass; that from the white portion of the vertical diagram is reflected from the upper surface of the plate, and is therefore uncolored[1]. The mixture of the two gives a light green field. For simplicity, we may assume that no light comes from the black portions of the diagram. In the portion of the light green field corresponding to the black of the vertical diagram, the white component will be wanting and the green will appear undiluted; in the portion cor-



[1] A small portion is also reflected from the lower surface of the glass, so contributes a small amount of green.

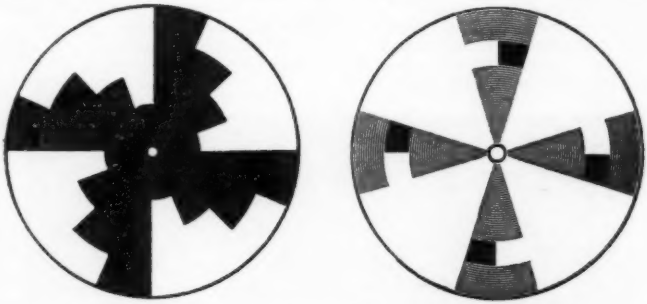
responding to the black of the horizontal diagram, the green component will be wanting and the faint white (*i. e.*, gray) should appear by itself. It does not, however, because of the contrast color induced upon it. As a matter of fact, the black portions are not absolutely black; the small amount of light that comes from them tends on one hand to make the green one (image of the black of the vertical diagram) a little whiter, and on the other hand to counteract the contrast in the purple one by adding to it a little green. Try the experiment with other glasses than green. (2) Another form of the mirror contrast experiment is as follows. Place a mirror where the sky or a white surface of some kind will be seen reflected in it. Lay upon its surface a plate of colored glass, green for example, and hold a little way above it a narrow strip of black cardboard or a pencil. Two images will be seen: one a vivid green, the other a complementary purple. The green image belongs to the surface reflection of the colored glass, as may be proved by observing that when the strip of cardboard touches the surface, the green image touches it also. The explanation will readily be understood from the accompanying diagram.



In the diagram, *A* represents a white surface, *C* the strip of cardboard, *E* the plate of green glass, *F* the glass of the mirror. As in Ragona Scinà's experiment, the white surface is reflected unchanged from the upper surface of the green glass. A good deal of the light, however, traverses the green glass and the mirror, is reflected from the back of the mirror, traverses the green glass again, and finally, as a strong green, mixes with the white reflected from the surface of the green glass, forming, as in Ragona Scinà's experiment, a light green field. The black strip *C* is reflected at *O*, that is to say, at *O* is a place where the white from the surface *A* is cut off, and only green from *M*, by way of *S*, is present, hence its image appears green. But *C* is also reflected at *T* (or its light is wanting there), so that the white reflected from *R* is unmixed with green. By contrast, it appears purple. It is easy, by substituting for *C* a gray strip that will send some light through the glass at *O* and *R*, to show that contrast can suppress an actually present objective color.

c. Meyer's experiment. Lay on a large colored field a small piece of gray or even black paper (*e. g.*, 1 cm. wide by 2 cm. long), and cover the whole with a piece of semi-transparent white paper. The contrast color will appear on the gray paper. If thin tissue paper is used, more than one thickness may be needed for the best result. R. Jung, Heidelberg, sells a book (for Becker's *Florversuche*) of alternate leaves of colored and tissue paper, with two gray rings attached, made expressly for Meyer's experiment. Paper mats, woven one way of gray paper and the other of colored, show this contrast beautifully, as Hering mentions. They may easily be made from kindergarten materials.

d. Mixed contrasts with the color-mixer. (1) Disks made on the pattern of the cut at the left show beautiful contrasting grays.



The same can be shown also by laying a number of small sheets of tissue paper over one another in such a way that they partially overlap, making a portion where there is but a single thickness, and next it a portion where there are two thicknesses, and next that again one of three thicknesses, and so on. When the whole is held up to the light, the contrasts of adjacent portions are very easily seen. (2) Contrast colors can be shown finely with disks like that in the cut at the right, in which the shaded portions represent color, the black portions black and the white, white. A little care is necessary in fixing the proportions of the color to white and black in the disks for contrast colors, but in general the brightness of the gray should be about that of the color.

On *a* cf. Helmholtz, *op. cit.* F. 517-519, 531; G.[1] 393-395, 405; G.[2] 551-553. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Die farbigen Schatten, Pflüger's Archiv, XL, 172. V. Bezold. *op. cit.*

On *b* (1) cf. Helmholtz, *op. cit.* F. 531-532; G.[1] 405-406; G.[2] 557-558. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Der Spiegelcontrastversuch. Pflüger's Archiv, XLI, 1887, 358-367. Wundt, *op. cit.*, I, 482. See also the physiologies in general.

On *b* (2) cf. Dove: Versuche über subjective Complementarfarben, Pogg. Ann., XLV, 1838, 158. Helmholtz, *op. cit* F. 532. G.[1] 406; G.[2] 558. V. Bezold, *op. cit.*

On *c.* Helmholtz, *op. cit.* F. 523, 530-531; G[1] 398, 404-405; G.[2] 547-548. Hering: Ueber die Theorie des simultanen Contrastes von Helmholtz; Der Contrastversuch von H. Meyer und die Versuche am Farbenkreisel, Pflüger's Archiv, XLI, 1887, 1-29.

On *d.* Helmholtz, *op. cit.* F. 538-543; G.[1] 411-414, G.[2] 544-547. Hering, *op. cit.* on *c.* V. Bezold, *op. cit.* Meyer, Pogg. Ann., XCV, 170, Phil. Mag., Ser. 4, IX, 547.

For quantitative measurements of contrast of grays cf. Ebbinghaus, Die Gesetzmässigkeit des Helligkeitscontrastes, Sitzber. d. k. Preus. Akad., Berlin, Sitz. v. 1, Dec., 1887. Lehmann: Ueber die Anwendung der Methode der mittleren Abstufungen auf den Lichtsinn; Die quantitative Bestimmung des Lichtcontrastes. Wundt's Philos. Studien, III, 1886, 516-529.

143. Conditions that influence contrast. *a.* **Contrast effects are stronger when the colors are near together.** (1) **Lay a bit of white paper on a black surface,** *e. g.*, **a piece of black velvet, and notice that the paper is whiter and the velvet blacker near the margin of the paper than elsewhere, notwithstanding that the eye moves about freely. This has received the name of "Marginal contrast" (*Randcontrast*).** (2) **On a piece of gray paper, the size of a letter-sheet, lay two strips of colored paper close side by side (*e. g.*, pieces of red and yellow or of green and blue, 1 cm. wide by 4 cm. long). Below them to the right and left as far apart as the paper will permit, lay two other strips of the same size and color, red on the red side of the former pair, yellow on the yellow side. Notice the effect of the difference in distance on the contrasting pairs. Contrast of this sort is at a maximum when one color entirely surrounds the other.**

b. **Effect of size. When the area of the inducing color is large and that of the induced color is small, the contrast is shown chiefly on the latter; when the two areas are of about equal size, as in *a* (2) above, the effect is mutual. Try with large and small bits of paper upon a colored field.**

c. **Borders and lines of demarkation that separate the contrasting areas tend to lessen the effect by excluding marginal contrast. Repeat Ex. 142*c*, using two slips of gray paper 5 mm. wide by 2 cm. long, substituting a piece of moderately transparent letter paper for the tissue paper. When the contrast color has been noted, trace the outline of one of the slips with a fine ink line upon the paper that covers it. Notice that the color nearly or quite vanishes. This experiment and others like it play an important part in the psychological, as opposed to the physiological, explanation of simultaneous contrast (see Helmholtz, *op. cit.* F. 533 f., G.[1] 406 f., G.[2] 559 f., but cf. also Ex. 144). Such a black border will, however, also make a weak objective color invisible. A disk like that in the cut accompanying Ex. 142*d*, when provided with a second contrast ring, marked off on both its edges with a firm black line, shows a weakening of the induced color in the bordered ring.**

d. **Saturation. Contrast effects are generally most striking with little saturated colors.** (1) **Compare the effect of increasing, decreasing and extinguishing the second non-colored light in the colored shadow experiments. It is necessary, however, to see to it that reflected light from the walls and surrounding objects does not complicate the experiment.** (2) **Compare the intensity of the contrasts in Meyer's experiment (Ex. 142*c*) before and after the application of the tissue paper. Notice also the part played by the white light mixed with the colored light in the mirror contrast experiments above. Powerful contrasts with the most saturated colors can be observed, however, when the proper conditions are fulfilled.**

On helpful conditions in general cf. Helmholtz, *op. cit.* F. 513-514, G.[1] 390-391, G.[2] 540-541.
On *c*. Helmholtz, *op. cit.* F. 539-542, G.[1] 411-414, G.[2] 546-547.
On *d*. Helmholtz, *op. cit.* F. 528-524, G.[1] 399-400.

144. Simultaneous contrast. The effects of simultaneous contrast are often lost in the more striking ones of successive contrasts, and the first requisite of an experiment on simultaneous contrast is the exclusion of the successive. This is not difficult in experiments in colored shadows. *a.* **Place a piece of white paper in such a position that it may be illuminated at once from the window (if the day be overcast) and from a gas-jet. Set upon it a small block or other object, about 1½ by 3 inches in size, and either black or white**

in color. Light the gas and observe the two shadows, one cast by the light from the window, the other by the gas. The first will appear yellowish, the second clearly blue.[1] Adjust the distance and position with reference to the light so that the shadows shall appear about equally dark, and the blue shadow shall be as sharply bounded as possible, and to that end have the shadow cast by the edge rather than the flat side of the flame. The color of the yellowish shadow is objective and due to the yellow of the gas-flame, that of the blue is due to the contrast, but largely as yet to successive contrast. Put a dot in the centre of the blue shadow, to serve as a fixation-point, and another on the edge. Fasten a paper tube (preferably blackened inside) so that it can easily be shifted from one dot to the other. Cut off the gas-light by holding a card between it and the block; adjust the tube so that the dot in the middle of the shadow may be fixated without any of the parts of the field outside of the shadow being seen. Wait until all of the blue has disappeared from the shadow, and then, still looking through the tube, remove the card. The field remains entirely unchanged and appears, as before, a colorless gray. The former blue color is thus shown to be subjective and due to contrast with the yellow lighted area in which it lies. *b.* Cut off the gas-light again and adjust the tube so that the dot in the edge of the shadow may be fixated. Taking great precaution not to move the eye, withdraw the card. The part of the field of the tube filled by the shadow will appear bluish, that of the remainder reddish-yellow. After a little time of steady fixation, cut off the gas-light once more and observe the instant reversal of the colors. The shadow now appears in reddish-yellow, the rest of the field blue. The color of the shadow, both before and after the final interposition of the card, is due to simultaneous contrast, in the first case with the reddish-yellow light, and in the second with its after-image.

Helmholtz explains all cases of simultaneous contrast as errors of judgment; in the case of the colored shadow, for example, we mistake the yellow of the gas-lighted field for white and consequently find the shadow which is really gray to be bluish. In the case of this particular experiment, Hering and Delabarre seem to have shown this psychological explanation unnecessary and a physiological one all sufficient, and Hering has done the same for other forms of experiments.

Cf. on simultaneous contrast in general. Helmholtz, *op. cit.* F. 515-547, G.[1] 392-413, G.[2] 542 ff. Hering, *op. cit.*, under Ex. 142. On colored shadows, cf. Helmholtz, *op. cit.* F. 517-519, G.[1] 394-396, G.[2] 551-553.

On Helmholtz's theory, cf. Helmholtz, *op. cit.* F. 533-538, G.[1] 392, 407-411, G.[2] 543 ff. Hering, *op. cit.* under Ex. 142; also, Ueber die Theorie des simultanen Contrastes von Helmholtz: Die subjective "Trennung des Lichtes in zwei complementäre Portionen."

Delabarre: Colored Shadows, AMERICAN JOURNAL OF PSYCHOLOGY, II. 1888-89, pp. 636-643. For quantitative measurements of simultaneous contrast, see Kirschmann: Ueber die quantitativen Verhältnisse des simultanen Helligkeits und Farben-Contrastes, Wundt's Philos. Stud., VI, 1890.

145. Simultaneous contrast. Hering's binocular method. *a.* Use the binocular color-mixer described above in the note on apparatus. Set a red glass in the right frame, a blue glass in the left. Look fixedly through the colored glasses of the instrument at the cork ball below, bringing the eyes close to the glasses and the

[1] This setting of the experiment succeeds best when the daylight is weak, as, for example, just before the lights are usually lighted in the evening. If the experiment is to be made in broad day, the light must be reduced by curtains or otherwise; if at night, there must be two lights, one corresponding to the window and one to the gas, and the latter must shine through a pane of colored glass. If yellow glass is used the colors will be the same as those in this experiment.

nose between them. Adjust the side screens till the white ground below appears in a uniform light violet from the binocular mixture of the red and blue (cf. Ex. 153). The narrow strip of black paper on the white is seen double, the right hand image bluish, the left yellowish. *b*. The possibility of successive contrast is, however, not yet excluded. That may be accomplished as follows: Lay a sheet of black paper over the whole of the white field and its black strip; rest the eyes; and finally, when everything is in readiness, and the eyes again fixed on the ball, swiftly draw away the black paper. The contrast colors are seen on the instant, before any motions that might introduce successive contrast have been made.

Hering argues that this experiment is conclusive against the psychological explanation of simultaneous contrast, unless a separate unconscious judgment is to be made for each eye, for that which consciously appears is a light violet field, and the contrast color to that should be a greenish-yellow, and both images of the strip should be alike, whereas, as a matter of fact, the images appear in different colors, neither of which is the color required.

Hering: Beitrag zur Lehre vom Simultankontrast, Zeitsch. f. Psychol. I, 1890, 18-28. For a different experiment supporting the same conclusion, see Hering's paper.

146. Influence of judgment in visual perception. While in the previous experiment a psychological explanation seems sufficient for the facts, psychical action is not excluded, even by Hering, from a considerable share in sense perception. In the following experiments judgment coöperates in the result. *a*. Place upon the color-mixer a short-pointed-star of white cardboard, or even a square, when in sufficiently rapid rotation, it appears as a white central circle surrounded by a more or less transparent ring. While it is in rotation bring behind it a broad strip of black cardboard of somewhat greater length than the diameter of the star from point to point. As the edge of the card advances it can be seen not only behind the transparent ring, but, apparently, also behind the opaque central circle, and the portions of the latter in front of the black card seem darkened by its presence. The illusion holds, though with a lightening instead of a darkening effect when a white card is moved behind a black star. The illusion fails by degrees if the card is kept motionless, but may be observed to a certain extent when the star is at rest. *b*. Cover a piece of black cardboard smoothly with tissue paper and notice that it seems, at first, blacker than it afterwards proves to be on comparison with other grays. *c*. In mixing colors by reflection (Ex. 140*a*), notice the tendency to see one color through the other instead of seeing the mixture of the two. This tendency may be so strong at first as to interfere, to a certain extent, with the success of the experiment. Cf. also Ex. 152.

On the difficulty of judging small differences in the color of surface that present other small unlikenesses, cf. Hering, *op. cit.* under Ex. 142*c*.
On *a*., Sanford, *Science*, XXI, 1893, 92.

147. An effect exactly the reverse of contrast appears when a figure in black or white is placed upon a colored ground. The black figure appears to darken the ground and the white to brighten it. This is a method often used in polychromatic decoration. Observe the effect on the blue ground in Fig. 4, Plate I. It may be observed occasionally in plaid fabrics, and is shown very satisfactorily in kindergarten mats woven in checker-board pattern of colored and gray papers. If a set of grays is used so that the strips may range from a black at one side to a white at the other, the corresponding shading of the colored paper is striking.

V. Bezold, *op. cit.* 182-183, and Plate V.

Binocular Phenomena of Light and Color.[1]

148. In general the two eyes coöperate to bring about a single visual result, but the union of the impressions upon the two retinæ is influenced by a number of circumstances. *a.* If the stimulus to one eye is considerably stronger than that to the other, the sensation in the latter is in most cases totally suppressed. Close one eye and look at a sheet of white paper with the other, letting the open eye move about freely. There is no tendency for the darkened field of the closed eye to assert itself. *b.* When, however, the effect of the stimulus in the open eye is somewhat weakened by steady fixation, such a tendency is to be observed, and the whole of the field of the open eye, except a small area about the point fixated, may be, from time to time, suppressed by the dark field of the closed eye. A slight motion will, however, instantly restore the first. Cf. also Ex. 118. *c.* A field that contains sharply marked objects or contours will generally triumph over one that does not. Try combining the letters below in such a way that the B's are superposed. In these the white field of either eye which corresponds to A or C in the other eye will generally not triumph over the letters.

A B B C

On the Binocular Phenomena of Light and Color in general, see Fechner: Ueber einige Verhältnisse des Binocularen Sehens, Abhandlungen der kgl. sächs. Ges der Wiss. VII. 1860, pp. 339-564. Helmholtz, *op. cit.* P. 964-999, G.[1] 767-796. Hering: Hermann's Handbuch der Physiol., III. Th. 1. 380 385, 576-577, 591-601 ; Beiträge zur Physiologie. 308-316. Aubert: Grundzüge der physiologischen Optik, 499-503, 550-554. Wundt: Physiol. Psychol. 3te. Aufl. II., 177-179, 183-189. Ebbinghaus: Ueber Nachbilder im binocularen Sehen und die binocularen Farbenerscheinungen überhaupt, Pflüger's Archiv, XLVI, 1890, 498-508. Titchener, Ueber binoculare Wirkungen monocularer Reize, Wundt's Philos. Studien, VIII. 1892, 231-310. Chauveau: Several articles in the Comptes rendus, CXIII, 1891, 358, 394, 439, 442.

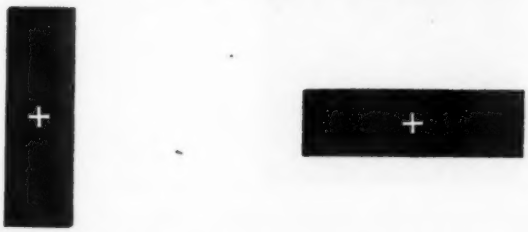
149. Fechner's paradoxical experiment. Hold close before one eye a dark glass, such as is used in protecting the eyes, or a piece of ordinary glass moderately smoked over, or even a black card with a good sized pin-hole in it, allowing the other eye to remain free. It is easy to see that the binocular field is darkened by the interposition of the dark glass. If, however, the eye behind the glass is closed, or the light wholly cut off from it by holding a black card in front of the glass, the field appears decidedly brighter, that is to say, cutting off a portion of the stimulus received by the total visual apparatus, has caused an increased intensity of sensation. The experiment fails for very dark and very light glasses. Several explanations have been given, but that of Aubert, according to which the sensations of the two retinæ blend in a sort of average result when the difference is not too great, but one wholly suppresses the other when it is very great, seems to be the most satisfactory, and with this Hering also in the main agrees.

150. Rivalry. When the two retinæ are stimulated separately with strong light of different colors, or are confronted with otherwise incongruous fields, *i. e.*, fields that cannot be given a unitary

[1] The experiments that follow can all be made with the stereoscope, but practice will enable the experimenter to combine the diagrams with free eyes, either by crossing the lines of sight (fixating a point nearer than the diagram), or by making them parallel or nearly so (fixating a point beyond the diagram). This skill the experimenter should try to acquire. In these experiments it is important that the eyes should be of approximately equal power, and if the poorer eye cannot be helped with lenses, the vision of the other must be somewhat reduced by the interposition of a sufficient number of plates of ordinary glass.

interpretation, there result a peculiar instability and irregular alternation of the colors over part or the whole of the combined fields of vision. This apparent struggling of the fields is known as *Retinal rivalry*. Hold close before one eye a piece of blue glass, before the other a piece of red glass, and look toward the sky or a brightly lighted uniform wall. The struggle of colors will at once begin. The same may be observed with a stereoscope when the usual paired photographs are replaced by colored fields, or even with no apparatus at all, when both eyes are closed and turned toward a bright sky and one of them covered with the hand. Rivalry has been explained as due to fluctuations of attention, and some observers find that it can be more or less controlled by attention (Helmholtz); Fechner discusses the attention theory, and finds it insufficient. Hering and others regard the changes as of a purely physiological origin. Cf. Ex. 151*b*.

151. Prevalence and rivalry of contours. By "contours" is here meant lines of separation where fields of one color border upon fields of another color. *a*. Combine stereoscopically the two bars below, and notice that it is the contours that suppress the solid parts of both the black and white. This figure gives excellent results when colors are substituted for the black and white.



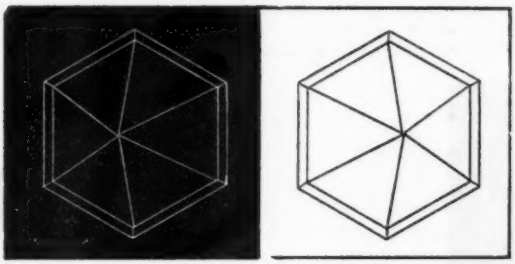
Notice a similar triumph of the contours of the cross over the lines, and of the lines over the central black of the cross in Fig. XI, or an enlargement of it.

Fig. XI Fig. XII

b. Notice the rivalry of the contours in all of these figures. *c*. Such diagrams as Figs. XI and XII are suitable for the study of the part played by attention in rivalry. While it is doubtful that mere attention to one field or the other will cause it to predominate, it yet seems possible by indirect means to cause it to do so. If attention is given to an examination of the lines and small squares in Fig. XI, or if one of the series of lines in Fig. XII is counted, they will appear to be somewhat assisted in their struggle with the cross or the other set of lines. *d*. A printed page has a decided advan-

tage. Try a diagram in which a printed page is brought into competition with a field of heavy cross lines. The lines will be found to yield to the print, at least, at the point at which the reader is looking. Two printed pages, however, become hopelessly mixed, and it is hard to say how much of the advantage, when a single one is used, is due to its superior power as a holder of attention, and how much to its excellence as a set of contours. A portion of the power of contours is probably to be explained by the mutual intensification of both the black and the white by contrast, but a part is perhaps due to a strong tendency, observable in other cases also, for the eyes (and attention) to follow lines, and especially outlines.

152. Luster. When one of the rival fields is white and the other colored (especially when one is white and the other is black), there results, besides the rivalry, a curious illusion of shine or polish, known as *Binocular lustre*. *a*. Examine in the stereoscope a diagram made like the accompanying cut, and notice the graphite-like shine of the pyramid. The explanation seems to be that polished sur-



faces, which at some angles reflect light enough to look white, and at others appear in their true color, have often in previous experience given rise to such differences of sensation in the two eyes, from which in this instance we infer a polish on the object seen in the diagram. *b*. A species of monocular lustre (or transparence) is to be observed when black or white or colors are combined by means of the reflection color-mixer, especially when the inclination of the plate is so changed that one color appears to be reflected in the surface of the other, or to be seen through and behind it. The experiment works well when real objects are reflected in the surface of the glass, the reflecting power of the latter appearing as if transferred to the horizontal surface on the opposite side.

153. Binocular color-mixing. The result of simultaneous presentation of different colors to the two eyes is not always rivalry or lustre. If the colors are not too bright and saturated and the fields are without fleck or spot to give one the predominance, a veritable though somewhat unsteady mixture of the colors may result. *a*. Place a red and a blue glass of equal thickness in the binocular color-mixer, and adjust the side screens till the proper amount of white light is mixed in with that transmitted from below. The mixture will be seen on the white field below. Try also with other combinations of glasses. The mixtures obtained in this way are not exactly the same in appearance as the monocular mixtures studied above. *b*. The same effect may be conveniently obtained with a stereoscope, from which the middle partition has been removed.

Try with equal areas of dull colors of little saturation. Hering recommends two squares of red and two of blue, set at equal distances in a horizontal line, the two reds on one side, the two blues on the other. When the middle pair are combined stereoscopically, they show a mixed color, while the unmixed colors can be seen for comparison beside them. He also suggests the use of lenses to prevent sharp focusing of the eyes upon the contours, which interfere with the mixture. Complementary colors are said to be more difficult to fuse than those standing nearer in the color scale. Cf. also Ex. 149. For diagrams that bring in binocular perspective to aid in mixing the colors and for a specially adapted stereoscope, see Chauveau.[1]

154. Binocular contrast. The side-window experiment. Stand so that the light from the window falls sidewise into one eye, but not at all into the other. Place in a convenient position for observation a strip of white paper on a black surface. The paper when looked at with both eyes appears perfectly colorless. On looking now at a point nearer than the bit of paper (*e. g.*, at the finger held up before the face), double images of the bit will be seen. The two images will be different in brightness and slightly tinged with complementary colors. The image belonging to the eye next the window (which may be recognized by its disappearance when that eye is closed) will appear tinged with a faint blue or blue-green color, the other with a very faint red or yellow. The light that enters the eye through the sclerotic is tinged reddish-yellow, and makes the eye less responsive to that color; the white of the paper strip therefore appears bluish. It appears darker partly for a similar reason, and perhaps also, as Fechner suggests, because it lies in a field which for the eye in question is generally bright. The reddish color of the other eye's image of the strip is explained as due to contrast with the first, but whether this contrast is a purely psychical matter, or whether it is to be explained by the action of the stimulus in the first eye upon the second, as there seems some reason to think, is as yet uncertain. Its greater brightness is probably due to the fresher condition of the eye to which it belongs, and to contrast with its less brilliant field. The same thing is often to be noticed when reading with the lamp at one side, or even when one eye has been kept closed for a short time while the other has been kept open. The double images are in nowise essential; simple alternate winking will show decided differences in the condition of the two eyes.

155. Binocular after-images. Lay a bit of orange-colored paper on a dark ground, and provide two white cards. Hold one of the cards close to the left eye, but a little to one side, so as *not* to hide the bit of paper. Hold the other eight or ten inches from the right eye in such a way as to hide the paper. Look at the paper for a few seconds with the left eye, then bring the card before it. A faint, washy, orange-colored positive after-image will appear on the card before the right eye. This after-image is supposed to belong to the right eye's half of the visual apparatus, possibly to the central, *i. e.*, cerebral, part.

[1] Comptes rendus, CXIII, 1891, p. 442.

NOTES AND LITERATURE.

NOTE ON COLOR-HEARING.

In 1887 I met a remarkable case of color-hearing and made quite full notes. The matter was one of which, at that time, I had never heard, and thinking I had a new field for study, I went hunting for victims, subject to pseudo-chromesthesia. I studied some blind cases and had a theory. Just then I discovered Galton's Study upon the same subject and dropped the matter. I made a second observation upon my original subject in 1891, and this spring made a third one, and believe the case of sufficient interest to make a record of it.

I have no theory to offer. I do not agree with Mr. Galton's, however, but consider one of the physiological explanations preferable. When I first studied the subject, I believed that the phenomenon was due to crossing or mingling of fibres of the auditory and visual nerves—a theory which I see has been held by some writers. I am not qualified to urge the theory or reject it; but I am heartily thankful to Mr. Krohn for his late valuable synopsis of the subject, and its literature.

G. L. is a young lady of unusually quick and bright mind; she has some artistic power and is a writer of ability. From early childhood she has had color-hearing. When a very little girl she was laughed at because she said that names were colored. That in her case it is truly sound that is colored is shown by the fact that names were colored before she could spell or read at all. At the same time she has mental imagery of great clearness and now *sees* the colored words when she hears them. Although all words are colored, letters, numbers and personal names are the most clearly and definitely affected. In my first test I gave many words like snow, sky, ink, grass, and the like, to see if natural quality of objects had influenced the result, but could not see that it had. Unfortunately different lists were used on the three occasions, and only letters, numbers and a few personal names occur in all these.

The letters of the alphabet bear the following colors for G. L.:

A. Dirty-white.
B. Radish-color.
C. Corn-color.
D. Dark-brown.
E. Cloudy-white or light-gray.
F. Light-gray.
G. Grayish-yellowish-white.
H. Dark-gray, greenish tinged.
I. Dirty-white.
J. Dark-greenish-bluish-gray.
K. Gray.
L. Yellow.
M. Geranium-red.
N. Red, with white tinge.
O. Pearl-white.
P. Deep-bright-green.
Q. Light-gray-green.
R. Rich-reddish-brown.
S. Sunshine-yellow.
T. Blue.
U. Gray-white.
V. Radish-color and brown.
W. Red, with white cloud.
X. Brownish-beet-color.
Y. Dirty-white.
Z. Deep-beet-color.

This list is that of 1893. Comparing it with the earlier ones, I find no important differences. The only real variants are as follows:

1887.	1891.
D. Dark-brown.	H. Dark-stone.
G. Pearl.	Q. Light-green, yellow-edged.
K. White, edged gray.	V. Pinkish-terra-cotta.
Q. Greenish-yellow.	Y. Yellowish-gray.
V. Burnt-sienna and yellow.	
Y. Yellowish-white.	
Z. Pinkish, dirty-dark-mottled-brown.	

We shall not attempt an analysis of the case, but will call attention to a few points. The vowels are all gray or white; M and N are almost alike; L and S are identical.

As to numbers:

1. White.	12. Corn-color, deeper-edged.
2. Blue.	13. Beet-color.
3. Radish-color.	14. Beet-color faded.
4. Brownish-radish-color.	15. Dark-green-gray.
5. Dark-gray-green.	16. Dark-green.
6. Dark-rich-green.	17. Light-sunny-yellow.
7. Sunshine-yellow.	18. Richer-deep-yellow, brown haze.
8. Cadmium-yellow.	
9. Dark-dull-gray-green.	19. Dark-dull, gray, green.
10. Pale-sunshiny-yellow.	20. Pale-gray-blue.
11. Corn-color.	

The variants are of little consequence:

1887.	1891.
5. Black, white-edge, misty.	10. Golden, much like 7.
10. White, yellow suffusion.	13. Radish-color.
15. Dark-gray.	17. Yellow, " not so glorious as 7."
20. Cadet-blue.	18. " Almost old-gold."

The radish-color of 3 is identical with that of B. The sunshine-yellow of S and L is that of 7 and 8. Notice in Alice later it recurs; it is plain that the L sound prevails. The influence of the first part of all the " teens " is plain in the above list.

Long lists of names were given in each test, and to all the answers came immediately and definitely. Forty-one names occur in all three lists. Of these only five can be considered variable at all. They are as follows:

1893.	1891.	1887.
Nancy—Dull-red.	Dark-dirty-radish.	Whitish-brown.
Susan—Bright-blue, (with yellow).	Radish (with yellow).	Blue (with yellow).
William—Faded-light-red.	Gray (buff tint).	White (red edged).
Clifford—Light brown (and corn).	Light-gray (and buff)	Gray.
Moses—Red (yellow lining).	Pale-red(and yellow)	White and gray.

Out of the forty-one, Moses is the only real disagreement. Notice how sounds combine and control. The N in Nancy is clearly

the red or brown foundation. S gives a yellow always in Susan. The yellow lines for S in Moses appear on the red ground of M. We have not room of course for the whole name list, but here are some of the simpler cases:

Mamie—Yellowish-red.
Julia—Radish-color.
Minnie—Light-red.
Laura—Light-yellow.
John—Vandyke-brown.
Henry—Gray-blue.
Harry—"A bright luminous blue."

Fringes, crosslines, cloud tints occur, and always interestingly.

Mary—"Pearl white, suggestive of red lines."
Jane—"A deep sombre uncompromising green."
Caroline—"Thin but clear deep, bright blue, showing sunshine lines behind."
Frederick—"Brown made lighter and colder by white."
Moses—"Red, with fine lines of yellow."
Joseph—"Gray, with slight, fine lines of corn-color and black."

It is frequently in these detailed cases that the agreement between different observations is the most striking.

In closing we desire to add a few notes made on blind subjects at the State College for the Blind, Vinton, Iowa, in 1887. We seemed to find among the congenital blind absolutely no conception of color, and no tendency to imagine it in terms of other sensations. On the other hand we believe that those who have become blind in childhood, after learning colors, very soon come to perceive color sensations in their hearing. We examined perhaps a dozen. Of these, three were born blind, or were blind from infancy. Of the rest, at least three had color hearing to a marked degree. The most interesting case was Miss N. This lady had pseudo-chromesthesia before she became blind. Musical notes are colored to her:

Do—Brown.
Re—Gray.
Mi—Light-red.
Fa—Black.
Sol—Real white.
La—Dark-red.
Si—Light-clear-yellow.
Do—Lighter brown than lower *Do*.

The alphabet is colored, also numbers. In her case color is plainly associated with a mental *picture*, for 10 is colored white (1) and black (0), and 11 is very white. So words and names are colored from their component letters. Ink is white (I), green (N), yellow (K). The tones of musical instruments are colored. Thus: the

Violin—Red.
Clarinet—Reddish-yellow.
Piano—Low notes, black; high notes, clear, like crystal or yellow glass.
Guitar—Blue-purple.
Flute—"Diamond clear."
Harp—Brown and blue.
Banjo—"Almost like violin, but darker."

Voices are distinctly colored: my own was "brown and gray, real light," while my little boy companion spoke in "real dark brown" tones. The months are colored and I could not detect any association influence from sky or weather. The color-sound and mental-imagery of the blind is, I am convinced, an interesting special field of study.

PROF. FREDERICK STARR.

The article in Vol. V, No. 2, of this JOURNAL concerning the experiments conducted under Prof. Jastrow's direction in reference to our estimation of verticals and angles, leads me to think that an observation of my own, which I have not seen recorded by any writer, may not be without interest to the readers of this JOURNAL.

Let the experimenter prepare a large sheet of paper thus: Set this in some convenient position, with its lower line horizontal against a dark background, the paper itself being well illuminated. The observer's eyes themselves should be screened from strong light. Let the observer lie down, resting on the left side, facing the paper, the plane of which must be about perpendicular to the line of vision.

FIG. 1.

Let him gaze at the black spot steadily for as long as may be necessary to obtain a strong *after-image*. This will be *thought* as shown in Fig. 2.

Let the observer now, with eyes still closed, roll over to the right; he will find that he *thinks* the image turning on an axis in the same direction, and when he reaches a position in which he is lying on his back he will *think* the position of the image as seen in Fig. 3.

If he turns still further until he is lying upon his right side, having made a ¼ revolution, he will *think* the image as presented in Fig. 4, it having made only a ¼ revolution.

FIG. 2.

FIG. 3.

FIG. 4.

If he turn with face down he will think the image as seen in Fig. 5.

If he turn one more quarter, one would suppose from what has preceded that he would think the image as shown in Fig. 6; but in fact somewhere between Figures 4 and 5 there is a change, and when he reaches the end of this last ¼ turn the image is thought as it was when he originally lay in this position on his left side. See Fig. 7.

FIG. 5.

FIG. 6.

FIG. 7.

This simple experiment seems to me to indicate that our judgments of verticality are determined largely by relation of our retinal images with the consensus of the feelings involved in balance or relation of our whole mass to the direction of the force

of gravity. How enormously complex must be the judgment-problems here involved, is apparent when we consider that each difference of position involves changes in some elements of the complex psychosis of balance.

I present this simple fact to the active workers in the field of experimental psychology, hoping that it may prove of service to those who are engaged in the study of these special phenomena.

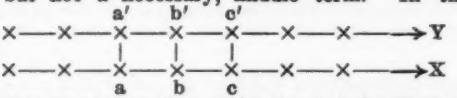
HENRY RUTGERS MARSHALL.

Zur Physiologie und Pathologie der Handschrift. GOLDSCHNEIDER. Archiv für Psychiatrie, XXIV, 1892, 503.

An important contribution to the theory of hand-movements. G. gives, first, a theoretical account of the origin of what the present writer has called "tracery-imitation," under the equivalent phrase, *malende reproduction*, *i. e.*, the association between visual pictures (letters, figures, etc.) and the hand-movements necessary to reproduce them (writing, drawing, etc.). He finds three "moments" in the rise of "tracery-imitation" (see his *résumé*, p. 587): A, an optical picture of the hand-movements (*optische Vorstellung der Handbewegung*) required for making the desired letter, derived from the child's earlier sight of his own hand-movements; B, a series of new motor discharges, strengthened by practice; C, a series of sensations of actual movement, by which the discharges are regulated and controlled. Moment A is clearly seen in the fact, often remarked, that in writing with the eyes closed we follow still a clear optical outline. In moment A there are two evident factors: (1), constant kinæsthetic memories (*Bilde*) from each position and each amount and direction of movement of the member, and (2) optical presentations of the same positions and movements (*optische intendirte Bewegungsbilde*). Moment C is for Goldschneider entirely kinæsthetic, as we would expect from his earlier papers.[1] He establishes, however, an entirely new element in this kinæsthetic complex in the case of writing, *i. e.*, a series of pressure sensations which vary with each character and each stroke. By a neat apparatus—consisting essentially of an air-cushion, upon which the pen-point of the subject rests, and a connected Marey tambour with graphic attachments—he demonstrates definite pressure curves for the different letters of the alphabet, punctuation marks, etc. [In the view of the present writer, this derivation of "tracery-imitation" is not adequate, simply for the reason that it assumes it. Given "moment" A—an association between definite hand-movements and the corresponding optical presentations of the same hand-movements—the latter might be sufficient, with practice, to innervate the former. But the question remains: How is this association established? How does the child come to connect the optical presentation of figure (*Gestalt*) with the optical hand-movement and kinæsthetic hand-movement series? G. does not recognize the fact that visual recognition of figure (pictures, letters, etc.) is definitely established long before the child is able or has any tendency to trace them.[2] He is wrong, accordingly, in identifying the optical figure series with the optical hand-movement series. An optical figure-series is really the first "moment" in the derivation of hand-writing—either a purely retinal series or an eye-movement series, according to the view adopted of the rise of visual perception of figure. The question then is: How does this optical figure-series come to stimulate the two muscular

[1] See *Untersuchungen über den Muskelsinn*, *Du Bois-Reymond's Archiv*, 1889.
[2] See my article in *Science*, XIX, 1892, p. 16.

series (kinæsthetic hand-motor and ocular hand-motor)? My observations show that the process is as follows: As the child's experience widens, its optical perception of figures grows exact; certain retinal or eye-movement series grows more and more fixed. At this period the arm and hand-movement series, at first few and fixed, are broken up with the increasing mobility of the member. Consequently, from the arm-movement pictures those elements are emphasized, (1) which are seen as well as felt, and (2) those from the latter which produce results identical with elements in certain definite figure-series already established by the eye. This reproduction of visual figure-elements by movements which are seen establishes gradually the association between the kinæsthetic movement-images and visual figure-outlines; the ocular-movement elements (*optische Vorstellung der Handbewegung*) being, no doubt, an auxiliary, but not a necessary, middle term. In the figure,



X= hand-movement-sensation series (*motorisches Bewegungsbild*), some of whose elements (a, b, c) at once reproduce (graphically) certain elements (a′, b′, c′) in the visual-figure series (Y= *optisches Bild des Gestaltes*). Now with the strengthening of this association by practice, appropriate arm (and hand) series follow promptly upon the eye (figure) series which constitutes the "copy." G.'s third "moment" is undoubtedly of great importance, whether or not it be entirely kinæsthetic.

The fact that the first moment is the visual-figure series, and not the visual-movement series, is seen (1) in the fact already cited that figure recognition arises first; (2) that we can trace figures, letters, etc., as G. says, with the foot, head, etc., by movements not usually seen; (3) the memories of the way the hand and pen look in writing are not at all clear compared with the two other elements, *i. e.*, the figure memory of the letters written, and the movement memories. For example, when I think of writing my own autograph, I picture clearly the figured letters which the point of my pen inscribes, and the muscular sensations of the hand, but hardly at all the way the hand looks in the successive stages of the signature; (4) in the analogous case of learning to repeat sound series, there are only two elements, the "copy" series—whether it be auditory (say the gutturals, which children sometimes learn first), visual, or speech-motor—and the resulting sound series (omitting G.'s third moment, the kinæsthetic control series as before); that is, there is nothing corresponding to G.'s optical-hand-movement series (*optische Bewegungsbilde*).]

Investigating, further, the "control" sensations—G.'s third moment—he finds, besides the pressure sensations mentioned above, that angular joint-movement sensations are most important. Beneath the threshold of perceptible joint-movement, pressure sensations still are felt, but pressure points are not distinguished. [This shows that some other control elements (perhaps central quantitative) are necessary for writing, executed by movements beneath the threshold of discrimination by joint sensations. Such writing has been demonstrated by Mohr.] By a further simple apparatus (see fig. p. 516), G. removes the resisting surface under writing movements, and finds that the curves of pressure for different strokes still remain, although the subject is not aware of giving different pressures.

On the pathological side, G. finds two classes of disturbances arising from impairment of the optical element and of the motor element. Under the former he cites left-handed writing, which is sometimes symmetrical with right-hand (*spiegelschrift*—from right to left), and sometimes a reproduction of the copy by the same movement as the right hand (from left to right). The former, "mirror-writing," he thinks is due to the tendency to symmetrical innervation of the two arms when one of them is practiced (*Mitbewegung*). It is possible only in as far as the optical "copy" image is thrown into the background or suppressed. The latter is due to the strength of this optical image in bringing both hands to the reproduction of itself. The motor disturbances of hand writing are also of two kinds, giving "trembling" (*Zitterschrift*) and "taxic" writing. The peripheral cause of the latter is impairment of any of the "control" series, visual, movement (joint), or pressure and resistance sensations. The central causes of disturbance he discusses in another paper (*Berliner klin. Wochenschrift*, 1892).

J. M. B.

Ein deutsches Gelehrtenleben von Prof. Dr. jur. J. E. Kuntze. GUSTAV THEODOR FECHNER. Leipzig, Breitkopf and Härtel, 1892, pp. 372.

The author of this volume is a nephew of Fechner's, and was for many years a member of his household.

Gustav Theodor Fechner was born in 1801 at Grossärchen, where his father had succeeded his grandfather as pastor of the church. His grandfather had won education and position by persistent struggle with poverty and discouragement, and his memory is still held in honor in the town where he lived as pastor over forty years. His father was remarkable for his independent and progressive spirit, which made him a leader among his people in matters of public utility as well as things spiritual. His mother was the daughter of a pastor, a woman of unusual force and sweetness of character. She was early left a widow in straitened circumstances with five little ones to bring up. The two boys were sent to an uncle, who provided for them for several years. In 1815, Frau Fechner was able to settle in Dresden, and the family was reunited for a short time. In 1817, Fechner entered the University of Leipzig to study medicine. He found most of the lectures he had planned to hear so unsatisfactory that he attended but two or three courses. Indeed, this dissatisfaction, combined with his distrust of the methods practiced in the profession, resulted in his giving it up altogether. He took the regular doctor's degree of the university, but devoted himself more and more to literary pursuits and scientific investigations. Even while studying in Dresden, he had partly met the expenses of his education by giving lessons and translating, and by these means he now earned sufficient for self-support. It was about this time (1820) that he read the first chapter of Oken's Naturphilosophie, which, to quote his own words, "so inspired me as to determine the direction of my mind for many years to come." To it he owed his escape from the atheism his medical studies had induced, and the permanent gain of the conception of all nature as a living unity.

In 1824 his mother and sisters joined him in Leipzig, and henceforth his home was with them until his marriage. He continued his translations from French scientific works, with voluminous additions of his own, and wrote besides many original papers on subjects connected with physics. To this period belong also his satires, which he published under the *nom de plume* of Dr. Mises. They are

marked by clear and admirable style, exuberant fancy and scathing wit, which spares neither philosophy, medicine, tradition nor convention. In the "Beweis dass der Mond aus Iodine besteht," as well as in some of the others, he gives humorous expression to the causes of his distrust of the methods then in vogue in medicine. In "Die vergleichende Anatomie der Engel" there are hints of conceptions later worked out more fully in "Nanna" and "Tend-Avesta." He began his lectures on physics at the university in 1824, and in 1834 received a full professorship. He had the most deep-seated distaste for any official restraint upon his intellectual life, and had only yielded to the solicitations of his friends in seeking the position to which he was finally appointed. A short time before he had married Fräulein Clara Volkmann, who proved a most faithful and devoted wife and companion, and who still survives him. The next six years were crowded with work, which Fechner felt obliged to pursue unremittingly, because of unavoidable family claims upon him for material aid. The crisis of his life was a natural result of this prolonged strain. In 1840 he gave signs of a complete nervous collapse, accompanied by severe head and eye troubles, which, for a time, completely disabled him. He has left a detailed account of this illness, believing that it may be of interest and service to others. The beginning of his gradual restoration to health of mind and body reads like the story of some miracle. Slowly but surely the improvement went on, and about 1843 he was once more able to resume his accustomed life and work with the joy and vigor of one risen from the dead.

During his illness a pension had been assigned him by the university, and although he never again assumed his former position, he gave voluntary lectures on the relations between mind and body for many years. He was not popular as a lecturer, except with a chosen few, for the average student craved instruction given on more limited lines than those on which his mind naturally worked.

Fechner's illness made the one serious interruption to his life-long intellectual activity. After his recovery he was able to devote himself to philosophic and scientific research and production for more than forty years. To his period belong his best known and most important works. It seems appropriate that the detailed exposition of his system of philosophy should be among the first fruits of this time. In 1848 "Nanna" appeared, and in 1851, "Tend-Avesta." In both of these he urges with eloquence and force the ground for his belief in that higher pantheism which was his deepest personal conviction. In nothing that he has written does he give greater evidence of his freshness and originality as a thinker, as well as of his rare literary gifts. His best-known contribution to science, the great work on "Psychophysik," appeared in 1860, and proved him a master in the new field, which had been but recently opened up. Wundt says "Fechner was the first to recognize the extent and significance of the investigations of Weber, and to him is due the creation of those exact methods without which further progress would have been impossible." The period of Fechner's interest and work in æsthetics, which now followed, is described by his biographer as a parenthesis. He threw himself into the subject with that ardor which characterized him in everything he undertook. His "Vorschule der Æsthetik" was the final exposition of his views, and in it he establishes a scientific basis for some of the fundamental ideas of beauty. The labor of his last years was chiefly given to the revision and strengthening of his work in "Psychophysik." Old age found him with unabated mental freshness and vigor. He was at work until his usual hour the evening that he

was taken ill. He lingered a few days, unconscious for the most part, but whenever he revived, his mind was perfectly clear. He fell quietly asleep, Nov. 18. 1887.

This life shows us a man endowed with a rare combination of qualities, both moral and intellectual. Simple and kindly as a child, indifferent to luxury—his one extravagance was in the matter of writing material—brave and patient in suffering, and afraid of nothing but idleness, of genuine piety and purity of life, he might almost have passed for a saint had he not been so remarkable as a scholar. Possessed of unusual independence, originality and versatility of mind, he yet had infinite patience in mastering subjects in themselves distasteful to him. Great as was his productiveness he did not write easily, and all his work represents painstaking revision and correction. Modestly conscious of his own powers, it is not strange that he was disappointed at the comparatively small recognition with which his philosophical system had met. Content with no subject until he had brought it into some sort of unity with his conception of the whole, he could ill understand the ability of many men to hold confused and inconsequent views. He was a true son of his people in his passion for nature, and his deep sense of man's close and intimate relaion to it. But he had his limitations. He was distinctly the nature-lover as distinguished from the lover of history and the past. Rome

> "A city was to him
> And it was nothing more,"

and he walked its streets far more interested in his own thoughts than anything outside them. For general social life and amusement he cared not at all. His one regular diversion was a weekly chess club, whose meeting he never missed, and he always enjoyed the society of intimate and congenial friends, and was often stimulated by them to discussion and argument, in which he took a keen delight. It is still too early to assign him his final place in the intellectual history of his country, but he is a connecting link between the past and present, and represents that scientific insterest in the physical side of mental phenomena which marks a new era in German philosophy. His great aim was to bring about the alliance of speculation and research, and in this good cause he fought loyally to the end. C. H. S.

Ueber die Gleichzeitigkeit und Ungleichzeitigkeit von Bewegungen." O. KÜLPE. Phil. Studien, 1891, VI, p. 514, and 1892, VII, p. 124.

Dr. Külpe discusses some experiments in which it was sought to raise both hands simultaneously. The subjects reached in four ways: ordinary muscular and sensorial reaction, and two unstimulated kinds, one with attention previously concentrated on the movement, as in muscular reaction, and one without special preparation. A bell furnished the stimulus (where necessary) in the first set; only such results were admitted as appeared simultaneous to the subjects. The figures given represent the deviations from simultaneity. In the majority of cases the left hand reacted first. The several subjects and kinds of reaction give widely varying results, but in general the figures for muscular and sensorial show the same relations as in simple reaction, and so do the unstimulated reaction —the average of the so-called "premeditated voluntary reaction" being slightly larger than that of the muscular, and that of the "unpremeditated" rather larger than that of the sensorial.

Looking at reaction from the psychological side, two laws of the association affect its rapidity. First, a period of strained attention

gives rise to a displeasurable feeling, which hastens transition. Second, the more closely states are related the quicker the transition. The coördinations or reaction are not natural, but in muscular reaction the *idea* of the coördination forms a natural connecting link. As to the former factor, in muscular reaction the attention is strained, hence the passage to movement is more rapid than in sensorial. In reacting with two hands, the deviation from simultaneity may be taken absolutely, or distinguished by plus and minus signs (*i. e.*, relatively).

To determine the causes for the priority of one hand, further experiments were made. First, with other kinds of stimulus: a hammer, light, and electrical stimulation of the skin. This produced no noticeable change of result. Next, the fingers of one or both hands were made anesthetic by ether or ice, thus removing the sensation of touching the reaction-key. This lowered the mean variation perceptibly, without altering the differences. In other experiments the efficiency of the muscles on both sides was diminished by a strong electric current; this increased decidedly the average priority of the left hand. To test the influence of *attention*, one hand was consciously singled out and attention directed more closely to its movement. Comparing these results with the others, there is a marked increase of preference for the left, when that hand is designated, and generally (but less noticeable) for the right, when it is designated. In some final experiments the muscles of one arm were fatigued by tension; the effect was to *delay* the action of that hand, especially in case of the right.

Dr. Külpe argues that, since the right hand is more accustomed to grasping and pressing than the left, it would usually press harder on the reaction-key, and (greater fatigue ensuing) would react more slowly than the left. But the left hand is more dependent; hence special attention to the right benefits it less than attention to the left. Chance directing the attention may therefore explain the variations in the earlier experiments. The larger variations in sensorial reaction may be because the preconceived idea of the movement represents only the transition from apperception to movement, while in muscular it includes the entire preparation for the movement, leaving less open to chance variation. In anesthesia the idea of the coming movement is not in conflict with the sensation of the present position of the hand. With the latter present, more or less of a counter-effort is required to prevent the movement from immediate accomplishment, and this increases the attention and fatigue.

The influence of this conflict between expectation and tension on the course and duration of reaction is to be investigated in another paper.

HOWARD C. WARREN.

The subliminal consciousness. MEYERS. Proceedings of the Society for Psychical Research, 1892, Feb. 1.

Holding that automatic writing, trans-utterance, automatic picture-drawing, crystal vision, monitory voices, hallucinations and kindred phenomena are in no sense abnormal, the author proposes a hypothesis for "the provisional coördination of all these subliminal phenomena," which "does not need constant stretching to meet the exigencies of each fresh case." Assuming that we must be in some sense conscious of any sensation or volition which we can afterwards recall, it is evident that multitudes of things have entered into consciousness without our knowledge. "Our habitual or empirical consciousness" is a selection of such parts of the whole

as have proved to be, in general, advantageous to the individual. It is related to the whole of consciousness much as the visible part is to the whole of the solar spectrum. The underlying psychical unit, the abiding "individuality" in each of us, is expressed, but always incompletely, as the "personality" of our ordinary waking state. At the lower, or physiological, end are processes that have become automatic; at the superior, or psychical, end are clairvoyant and other impressions which are habitually received, and which "do in some sense transcend the limitations, of time as well as space, within which all supraliminal consciousness necessarily falls."

Examples of changed personality suggest the possibility of such a psychical reorganization as shall incorporate into our ordinary personality powers now entirely subliminal, and impressions which now reach us occasionally as "messages" from the subliminal part of our individuality, which become visual or auditory or indefinite according to the character of the personality and the attending circumstances. Such "messages" may produce hallucinations which are objective in the sense that their source is not in the individual.

Dreams are a familiar example of slight changes of personality. Hypnotic trance, hysteria and insanity are other examples. The author adduces evidence in favor of his view that "subliminal consciousness" is entitled to the epithet "conscious;" and compares the contents of supernormal phenomena with what would be expected from his hypothesis. T. P. HALL.

Aufäuge und Aussichten der experimentellen Psychologie. KÜLPE Archiv für Geschichte der Psychologie, Bd. VI., Heft 2.

An historical outline. Contributions to *experimental* psychology have been made all along by physics and physiology; but the real question is, Why was psychology so slow in becoming an *independent* science? Because, first, of the neglect with which the "lower faculties" were treated; and again, because of Kant's unfavorable verdict, called forth by the empirical psychology of the 18th century. His objections were met partly by Herbart's mathematical psychology, partly by the actual founding of experimental psychology. This owes its existence, after Weber's suggestive work, to Fechner, who by demonstrating the functional relations between psychical and physical processes, did away with the inexactness of earlier psychologies, and by developing psycho-physical methods, supplied the necessary means of research. If his work is in some respects imperfect, and his estimate of Weber's law too high, it must be remembered, on the other hand, how scant was the material gotten up by his predecessor.

Wundt, the next leader, though at first under the influence of "pure" psychology, develops to the fullest, in his later work, the principle of parallelism. For him the correlative of psychical activity is the nerve-process, while for Fechner it is the outer stimulus. The advances made by these men within the domain of psychology proper, have been seconded by work in other branches, especially astronomy and physiology. As to pathology and zoölogy, important as their results may be, they offer no room for what is, strictly speaking, psychological experiment. E. PACE.